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Machiavelli's Political Science

HARVEY C. MANSFIELD, JR.
Harvard University

Machiavelli is presented as the founder of modern political science, with due regard to the fact that he never spoke of "political science." His usage of "prudence" and "art" in The Prince *is examined to see whether, as founder, he was a teacher or a ruler of future generations. His comprehensive attack on classical political science is outlined and developed through two essential points, the cycle and the soul.*

Modern political science presents itself today as both narrowing and progressive. Despite certain misgivings arising from the encounter with the New Left, most political scientists still put their trust in the fact-value distinction[1] as the method designed to narrow their range of concern and thereby to bring social as well as scientific progress. To an observer, the narrowing might seem more evident than the progress. At least it must be admitted that scientific narrowing has lost its evident connection to progress, since the very meaning of the fact-value distinction is that any good that might come of it is strictly accidental. To understand modern political science, therefore, one should look back to a time when this connection was argued in comprehensive fashion. Before investigating, one cannot exclude the possibility that this comprehensive argument was conceived, not in gradual stages, but *ad uno tratto* ("with one stroke") in the thought of *uno solo* ("one alone"). To set forth this claim on behalf of Machiavelli, with a view to our own self-understanding, is the purpose of this article.

Machiavelli's Modernity

We will not find the fact-value distinction in Machiavelli, for he passes value judgments right and left with unmethodical abandon. But we do find a realism that was ancestor or parent of the fact-value distinction in Machiavelli's famous call, in chapter 15 of *The Prince,* not to depart from what is done for what ought to be done. As opposed to making a profession of good in all regards, one should align one's values with facts in the sense of deeds. So the distinction is between deeds and professions rather than between facts and values, and the lesson is for the good of men, not for a methodological purity that cannot be proved to be for the good of anybody. For the sake of one's preservation (which is good), even for the common benefit of each human being, one must learn how to be not good. With this promise of preservation, Machiavelli connects his political science to progress toward the human good.

What one's preservation requires, according to Machiavelli, will become clear only gradually, but it may be glimpsed behind the meaning today of "modern," as for example in the phrase "modern political science." "Modern" as we use it today is defined against "traditional" so that what is modern constitutes self-conscious progress beyond tradition. But when modernity is established and what is "modern" becomes traditional, modernity must define itself against what had once bravely claimed to be modern. Thus, "modern" is always in danger of being surpassed by "more modern"; defined as against the traditional—that is, in relation to the traditional—"modern" seems to have no definition. Nor does "tradition": "traditional" is or supports the status quo, and "modern" advances—or merely moves—beyond it. "Modern," then, has perhaps a moving definition; it is always ahead of itself, not to say self-destructive. Or one might say that modernity has a certain direction.

In politics, more modern means further left and more democratic, as we can see in the alleged progression of the great modern revolutions—American, French, Russian, Chinese—each more democratic, supposedly, than the preceding one. What is more democratic, it is assumed, disposes of barriers between men, both humanly created and otherwise, as far as possible. Such barriers are especially the forms or formalities that define distances between men, give society a structure, and keep it from dissolving into an undifferentiated mass. Thus, modern art and literature have shown an inherent tendency away from classicism in perfection of form toward romanticism in concern for feeling that overrides form. Similarly, we have learned from Tocqueville that modern politics as a whole may be seen as the drive of extra-constitutional democratic forces to overcome the restraints of constitutional forms that were once

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[1] That science can establish facts but not values.

modern themselves.[2] The liberalism that was to set men free has been attacked and pushed aside by movements let loose by those dissatisfied with liberal formalism, who desired a more radical freedom: for the decline in respect for forms and formalities is accompanied by a demand for more subjectivity and creativity.

Such, in brief, is the course of the modern world as it races toward perfect democracy and freedom. But this sort of perfection is indefinable; as soon as one defines the modern, it becomes the status quo and hence traditional, an easy target for the next progressive. To understand modernity, therefore, one cannot look to its *end,* as it seems to have none; one must look to its *beginning,* when progress was first set in motion. Especially since modernity now seems out of our control, and "progress" no longer seems progressive, we need to know what was intended and hoped for originally.

When one looks to the periods at which modern history is said to begin, however, they do not appear to suggest, much less to launch, the characteristic dynamism of modernity. Humanism puts man, rather than God, at the center of attention—which does not necessarily imply progress toward a new earthly future for men; and the Renaissance is a rebirth, perhaps a return to the ancients. Only Machiavelli, a single man soaked in the Renaissance and steeped in Humanism, seems, of those in his times, to have declared himself for progress in terms we recognize.[3]

More precisely, Machiavelli is for novelty. In *The Prince,* he praises the new prince over the hereditary prince because the new prince depends only on himself and thus gains more glory; the highest case is the new prince in a new principality who is also the prophet of a new religion (*The Prince* 6, 24). And at the beginning of the *Discourses on Livy,* Machiavelli, comparing himself to those in his times who sought unknown seas and lands, says he is bringing "new modes and orders" to mankind. He appears to be the first political philosopher not merely to admit his own novelty (as for example Marsilius) but even to flaunt it; and he did this not merely to establish his new modes and orders but, in accordance with them, to give new reputation to those who seek out new acquisitions.[4] It is not that Machiavelli invented new political tricks. He admits to having borrowed techniques of government from the ancients and perhaps also from Christianity, and he was aware that before him there had always been half-hearted or untaught Machiavellians in need of instruction.[5] Rather, he believed that political men should be encouraged to make their own innovations so as to increase opportunities for glory and gain for themselves and their peoples, for "preservation" requires both glory and gain. The founder of a state does not legislate once in the hope that his forms will endure; instead, the healthy state must be made and remade, formed and reformed, or it will become corrupt.[6] Machiavelli is far from espousing a formless or stateless society, but he favors frequent reform and he is definitely no respecter of formalities.

Machiavelli's realism unites with his desire for innovation when one sees that learning how to be not good means especially learning how to introduce an innovation.[7] In the chapter of *The Prince* where he calls for learning how to be not good, he announces publicly that he departs in this "from the orders of others." Machiavelli thought he lived in times when men were weak and vile (at least in Italy); he spoke scornfully of modern politics and religion and by contrast appealed to "ancient virtue."[8] Such expressions might appear to put Machiavelli with the Renaissance and to demand a return to the ancients. But Machiavelli, we see, was very far from being reactionary. He rejects the authority of the ancients in this chapter, for they, together with the Christians, were the "others" who based their political science on what should be done rather than on what is done, who elaborated the "profession of good" in all regards, and who therefore constructed imaginary republics and principalities such as Plato's *Republic* and St. Augustine's *City of God.*

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[2] Tocqueville (1969, p. 698). Consider the criticism of liberal formalism in Karl Marx, "On the Jewish Question," in Marx (1975, pp. 219, 234).

[3] See n. 55 below and *The Prince* 12 (*progressi grandissimi*). Two recent works emphasizing Machiavelli's republicanism have lost sight of his progressivism. In Pocock (1975, pp. 158-77, 218), Machiavelli's concern with innovation is given due emphasis with regard to *The Prince,* but denied with regard to the *Discourses;* and see Skinner (1978, Vol. 1, pp. xiv, 45, 179; cf. Vol. 1, p. 181). See also Gerschmann (1973, p. 175).

[4] Thus, although new acquisitions are useful to princes and peoples, they must mainly appear glorious in order to attract glory-seekers. Machiavelli would have found utilitarianism too staid to be useful. Compare what he says about *cose nuove* in *The Prince* 6 with his remark on *cose presenti* in *The Prince* 24; and note *innovare con nuovi modi li ordini antiqui* in *The Prince* 7. Marsilius of Padua, *Defensor Pacis* I.I.3; I.XIX.3.

[5] See especially *Discourses on Livy* I.27 on *knowing* how to be altogether bad or good. On Machiavelli's borrowing from the ancients, see *The Prince* 3 (praise of Romans), 6 (beg.), 19 (end); *Discourses* I, proemium, 2, 4, 5, 21; II.16, 24, 33; III.2. On his possible borrowing from Christianity, consider *The Prince* 7 (on "Remirro de Orco"); *Discourses* I.52; II.16-18.

[6] *Discourses* I.6, 16-20; III.1, 3, 7. Note the progression from *The Prince* 4 (end) to *The Prince* 6 (on form and matter) to *The Prince* 25 (end). But refounding "every day" would be offensive, *The Prince* 8.

[7] *The Prince* 8 (end), 17, 18, 19 (end); *Discourses* III.35.

[8] *Discourses* I. proemium, 12, 55; II.2; III.1; *The Prince* 3, 12, 26; *Art of War,* proemium.

In regard to political teaching, the strong ancients are at one with the weak moderns. So in the preface to the first book of the *Discourses on Livy,* Machiavelli indicates that he will honor ancient politics through ancient histories, not by imitating ancient political science. Moreover, the history he chooses is Roman, not Greek; and near the beginning of the *Discourses on Livy* he reveals a definite preference for Rome over Sparta, the Greek city most favored by political philosophers. While making use of Polybius and Livy, he ignores Polybius' statement of indebtedness to Greek political science; and his use of Livy, of which much has recently been said, does not indicate acceptance of Livy's interpretations of events, to put it mildly.[9] Machiavelli puts his own interpretation on ancient virtue so that it becomes Machiavellian *virtù.* He values the large and imperialistic Roman republic above the Greek cities, and judges its virtue by its fortune in war, although that virtue was exercised in conquering the Greek cities among others.[10] While bowing ironically to the authority of the ancients—so that he can use it against the authority of the moderns—Machiavelli in fact uses ancient examples to reproach ancient teachings. He returns to the ancients in order to improve on them.

Unless one dissolves Machiavelli's arguments into phrases and reduces his design to vulgar office-seeking, one cannot find another thinker or statesman in his times or before who reminds us so vividly and profoundly of the realism and dynamism of modernity. Today, many would perhaps agree with this judgment, if not this formulation. But there remains a great reluctance to admit, or even consider, that Machiavelli might be chiefly responsible for the spirit of modernity and thus is himself the origin of the modern world. Modernity now seems so powerful and all-encompassing that it appears to be unstoppable, and if unstoppable, apparently inevitable. Few are ready to believe that the modern world, which wishes to move in the direction of perfect freedom for men and to give men ever greater control over themselves and the world, could have been founded by the free act of a human being. Strangely, we find it more comfortable to believe in confused and contradictory "forces of history," relishing our fate since we cannot maintain our hopes.

Before estimating what Machiavelli's influence might have been, one must see what he intended and what influence he intended for himself. One may begin from a typical view of Machiavelli's realism, which is that he believed that morality can be one's guide in private affairs but not in politics. Such a view seems implied in a remark of Thomas Hobbes', apparently directed at Machiavelli: "Successful wickedness hath obtained the name virtue . . . when it is for the getting of a kingdom" (*Leviathan,* Ch. 15). It is supported in Machiavelli's *Discourses* I.9, where Romulus' killing of his brother is excused because it was necessary for him to rule alone to order a kingdom or a republic for the common good.[11]

Yet, returning to chapter 15 of *The Prince,* we find that the title mentions "men and especially princes," while the principal advice is addressed to "whoever understands." Actual princes may be imprudent; indeed, according to Machiavelli, their sins are responsible for Italy's plight.[12] They must be replaced by prudent princes, who are now private men. But a private man must behave like a prince because a private man, if he is prudent, must become a prince—as Hobbes' remark suggests. Machiavelli's own suggestion, in his punning use of the word *privato,* is that a private man should regard himself as *deprived* of office.[13] Perhaps the rules of politics are not those of private morality, but when private men are compelled to become princes, they no more than princes can live by professions of good. Moreover, the prudent, private man who wants to become prince must not only be ready to make his way by such crimes as fratricide; he must also pave the way by making it easier for such crimes to be accepted as necessary. In *Discourses* I.10, just after providing an excuse for Romulus, Machiavelli shows how a founder is affected by his reputation, in particular how he can be hampered by an evil reputation as a tyrant. Machiavelli may have excused Romulus, but others—for example, St. Augustine—have not.[14]

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[9] *Discourses* I.5-6. Polybius VI.5.1; Livy IX.36.3; XXXIX.8.3. Strauss (1958, Ch. 3); cf. Gilbert (1977, pp. 115-33).

[10] *Discourses* I.9, 53, 59; II.1, 10; III.16; *The Prince* 3.

[11] Cf. *Discourses* I.18 (end), where Romulus' killing is excused without reference to the common good. See *Florentine Histories* I.3, regarding Attila, who also killed his brother in order to "be alone in the kingdom."

[12] *The Prince* 12; *Discourses* I.21; II.18; III.29.

[13] *The Prince* 2, 6, 7, 8, 14; *Discourses* III.2, 5. Note also the phrase *privata fortuna* in *The Prince* 6, 7, 12, 14. The "private citizen" who must seek office must also have the princely "humor" (*The Prince* 9) to be sure; but this fact does not justify the private lives of those who share the popular humor, who as such are deluded. See Price (1977, p. 620).

Thus, the prudent prince needs a whole new climate of political and moral opinion to facilitate his arrival and maintenance in power. This is what Machiavelli intends to supply. The prince, or founder-prince, "is alone" only if he alone sets the standards by which he is judged and his reputation is made. Only he who sets these standards is alone above ordinary moral persons who live by them and above princes who may share them and who must conform to them, or appear to conform, for the sake of reputation. The highest prince, who is in the fullest sense prince, is the moral or political philosopher who establishes the opinions in which lesser princes operate.[15] If, according to Machiavelli, public and private immorality must be controlled by the political necessity of acquiring and maintaining a state, then the highest prince is the political philosopher or scientist, Machiavelli himself—the one who brings "new modes and orders" for the common benefit not merely of Florentines or Italians but also of everyone.

Prudence and the Art of War

In this prospectus for "new modes and orders" establishing a new climate for prudent princes and private men, however, there is an evident difficulty. Is the end of the political scientist to pave the way for princes (thus also improving the lot of peoples), or, since the political scientist too is in a sense a prince, is his end to be prince himself? In the former case, Machiavelli's political science would be teachable to all and his status as teacher of all for the common benefit would be superior to his ambition of ruling as "one alone." He would indeed be teacher rather than prince, for his discoveries, like those of any scientist or philosopher, would not carry along to present and future beneficiaries the personal rule of their discoverer. The scientist, as teacher, is not strictly "one alone," and if he discovers "new modes and orders," his glory is merely to have been the first to see, and not now to be the first in rule, as prince.

Yet this formal truth regarding teaching in general seems overborne by the content of Machiavelli's particular teaching. When the teacher's lesson requires rising to sole rule, it seems unreasonable that the teacher should except himself, and unlikely that he will. In this case, his status as teacher would be subordinate to his ambition as prince, and the prudence he exercises in his own interest would control the science or art that he claims to be in the common interest. How can one sincerely advise "successful wickedness"?[16] If wickedness succeeds for those advised, then why not also for the adviser? And if the advice is wicked, why should it help those advised? Although Machiavelli's difficulty can be sensed by anyone of ordinary moral experience, it is obscured by scholars who make excuses for Machiavelli, conceal the wickedness of his advice and thus blindly rob him of the glory he claimed for having begun the scholarly practice of making such excuses. To excuse Machiavelli is to dismiss not only every popular, but also every interesting, sense of the word "Machiavellian."

In the dedicatory letter to *The Prince,* which was written to be understood "in a very brief time" by an actual prince, this difficulty of Machiavelli's political science appears in the relation between art and prudence, and the word "science" does not occur. Indeed, for some reason Machiavelli does not speak at all of "political science." "Science" is discussed in the *Discourses* (III.39), which were addressed to potential princes with more time on their hands. In this summary treatment, we shall mainly consider *The Prince.*

Machiavelli's dedicatory letter offers to Lorenzo his "knowledge of the actions of great men," which is the gift of everything he knows. From this he dares to "discuss and give rules for the government of princes" and says he possesses knowledge of the nature of princes. What he knows of the government of princes seems to be practical, and what he knows of the nature of princes, theoretical. These two kinds of knowledge appear to combine in the third chapter, where "the Romans" are cited as the example for wise princes of overcoming the particular difficulties of acquiring and maintaining a new principality. The Romans knew the remedies for these difficulties because they knew them "at a distance" (*discosto*) or saw ahead.[17] Such knowledge is likened to that of physicians taking timely measures to cure consumption, but is then limited to "a prudent one," as opposed to "the wise men of our times," and said to be "given" only to that one. The same foresight is recommended in the sixth chapter to "a prudent man"; he should imitate the great men who have not imitated anybody and have made altogether new principalities. He should behave like "prudent archers" who shoot at a mark above the target in order to hit the target. Where one would expect "skillful archers" one finds "prudent archers," as if prudence, understood as discounting ahead of time, were assimilated to art and art thereby given command over the future.[18] He who follows this procedure is said first to have "some odor" of the great men he imitates, and then, because archers know the virtue of their bows, and use the aid of the high mark, they are allowed to "succeed in their design." In a turn typical of Machiavelli's writing, the (singular) prudent man whose prudence is "given" to him becomes the (plural) prudent archers whose learned prudence supplies the lack of highest virtue. We are made aware of, but not directed to, the difference between the prudence of imitating great men and prudence which is similar to possessing an art. When imitating great men, one follows their beaten track and thus does not truly imitate their innovation; however, each archer, as such, is as "prudent" as any.

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[14] St. Augustine, *De civitate Dei* XV.5.

[15] One cannot create opinions altogether, since ordinary morality remains and will remain unaffected even by Machiavelli's instructions; see Mansfield (1979), on *Discourses* I.10. In *The Prince* 15, "the orders of others" from which Machiavelli departs refer to qualities for which men are praised and blamed. See also *Discourses* I, proemium; *Florentine Histories,* proemium.

[16] Or, how can one be a "teacher of evil"? Strauss (1958, p. 9). Lefort (1972, pp. 260-62) has read past the first sentence in Strauss' book and has taken note of the "if" in that sentence.

[17] See also *Florentine Histories* VII.5.

Machiavelli alludes to this difference at the end of the ninth chapter when he remarks that "a wise prince" must think of a "mode" through which his citizens will always have need of him and thus will always be faithful to him. Can a wise prince do this with political science? At the end of the tenth chapter, Machiavelli says he has solved the problem of keeping citizens loyal, at least in part, for "a prudent prince." In the first part of *The Prince* (Chs. 1-11), on the kinds of principalities and how to acquire them, he joins theory and practice or wisdom and prudence, while leaving unclear whether they must be given as prudence or may be learned as an art.

The second part of *The Prince* (Chs. 12-14), on the necessity of using "one's own arms," heightens the difficulty. Machiavelli condemns the use of mercenary arms but praises a certain mercenary captain in the service of the Florentines, Paulo Vitelli, as *uomo prudentissimo* for having risen from private fortune to very great reputation. If he had taken Pisa for the Florentines, Machiavelli remarks, the Florentines either would have had no recourse had he left them or would have had to obey him. Machiavelli does not remark that in fact this "very prudent man" did not take Pisa and was thereupon killed by the Florentines. They spied the danger to them in Vitelli's prudence, since "mercenary arms" to them were "his own arms" to him. But they also did not know how to take advantage of his prudence, such as it was. In chapter 13 Machiavelli deplores "the lack of prudence in men" by contrast to the wisdom which is given to the few, and he criticizes the Romans whose prudence he had praised in chapter 3. Among the few wise who order their own arms, he names Philip of Macedon, whom he had cited in the preceding chapter as a mercenary captain who took away the liberty of his employers. Machiavelli says he gives himself over entirely to Philip's (and to others') orders. Does Machiavelli then admit that the few wise, perhaps including himself, hire out their wisdom and rob their employers in the manner of mercenary captains?

Machiavelli answers this question provisionally in the fourteenth chapter of *The Prince.* Although the chapter heading does not mention the art of war, Machiavelli suddenly announces that the art of war should "therefore" be a prince's only object and only thought.[19] Whence the "therefore"? If the dubious mercenary captain named at the end of chapter 13, to whose orders Machiavelli gives himself over entirely, can be presented as teaching an *art,* then his wisdom can be made to appear as benefaction. Such is the virtue of this art that it not only maintains those born princes but also often causes men to rise from private fortune to the rank of prince. When princes have thought more of delicacies (*delicatezze*) than of arms, they have lost their state. Using a weighty phrase, Machiavelli says that the "first cause" of losing one's state is to neglect the art of war, and that the cause of gaining it is to be "professed" in the art.[20]

This is illustrated by Francesco Sforza, who through being armed became Duke of Milan, and by his sons, who through avoiding arms became private men.[21] The example of the Sforzas does not in truth illustrate the principle because it fails to mention the art of war; besides, Francesco, however proficient in that art, did not "profess" it in the sense of teaching it. Nonetheless, as we have seen, a mercenary captain who fights on all sides for hire and gains his own personal "state" illustrates our problem of art and prudence. Is his art so powerful as to be the first cause of his rise or does he also need his prudence to attune his art to circumstances? In this statement, the art of war has swallowed up prudence. Machiavelli goes on to say that no proportion exists between one who is armed and one unarmed, and that it is not reasonable for the armed to obey willingly whoever is unarmed. Thus, Machiavelli concludes, a prince who understands nothing of militia, among all his other misfortunes, is not trusted by his soldiers. He seems clearly to deny here the fundamental principle of classical natural right, the rule of the wise: it is not reasonable for someone who is unwise but armed to obey one who is wise but unarmed. Yet if the art of war is understood in its full extent as the only object and thought of a prince, Machiavelli in truth affirms the classical principle, while improving on it. For to understand "militia" or to possess the art of war is sufficient to make one armed, and to be armed is sufficient to make oneself obeyed, at least by one's soldiers and perhaps also by one's subjects.[22] Contrary to the classical writers, Machiavelli argues that knowing leads to commanding, and so the art of war in the extended sense includes politics. Nowhere else does Machiavelli give the art of war such amplitude.[23]

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[18] Aristotle, *Nicomachean Ethics* 1139b7-8, 1140a18.

[19] *Quella è sola arte che si espetta a chi comanda:* the article before *sola* is missing perhaps because Machiavelli is at a loss between the definite and the indefinite. Russo's suggestion, assigning *sola* to *chi comanda,* does not make sense; Machiavelli (1963, p. 125n.).

[20] Machiavelli claims to be "professed" in the art of war in *Art of War,* proemium. See also *Discourses* III.13. He does not claim to be a philosopher (*Discourses* I.56) and characteristically uses weighty philosophical phrases only in political contexts.

[21] Elsewhere Sforza is scolded by Machiavelli for having built a fortress which caused his heirs to lose their state; *The Prince* 20; *Discourses* II.24.

Moreover, he says next that a prince (or one) must never lift his thought from the exercise of war, and in peace more than in war. Never?[24] Does this mean that thought (or at least a prince's thinking) should never be detached from advantage in war? Does "effectual truth" mean effectual in war, whether foreign or domestic, and is this the only truth one should seek? Machiavelli's elaboration of this startling "never" seems to take away from its naked force, but only at first. He says that the exercise of war in peace is done in two modes, with works (*opere*) and mind (*mente*). But apart from keeping one's own arms in order, he says that a prince should always be on the chase so that he can accustom his body to hardships and learn the nature of sites. Machiavelli then discusses this topographical knowledge, of which one should take "the greatest care." The "works" of exercising war in peace seem hardly distinct from "mind."

Knowledge of sites is useful because it enables one to understand what is required for self-defense and because the topographical features in one region have a "certain similitude" to those in other regions. Knowledge of one site thus enables one to comprehend any other site. Such knowledge, the "first part" of a captain, is accompanied by reasoning, as Machiavelli makes clear by the example of Philopoemen, a prince who used to ride in the country with his friends and pose hypothetical questions to them concerning all the chances (*casi*) an army might encounter. With these "continuous cogitations," no accident could ever arise for which Philopoemen did not have the remedy: again, a sweeping claim of efficacy for the art of war.[25]

This "first part" of a captain seems equivalent to the "firm science" Machiavelli discusses in *Discourses* III.39, also on the knowledge of sites, where he also praises hunting.[26] He says that Xenophon (in his work that Machiavelli mistitles "Life of Cyrus") makes it clear that hunting is "an image of a war"; and Machiavelli adds that for great men this exercise is honorable and necessary. It appears that particular knowledge of one country can be generalized and the familiar made applicable to the new by the use of images representing the similitudes mentioned in chapter 14 of *The Prince*. By contrast to that chapter, *Discourses* III.39 speaks of both images and sciences so as to suggest that images (in addition to similitudes) are necessary to science (as beyond mere art). It was "firm science" that on one occasion enabled a Roman tribune to save the Roman army, despite the bafflement of the consul in charge, by spying a summit above the enemy on which both he and that army could take refuge. An image, then, might be a similitude visible only to one person or to a few, for sometimes it takes a rare brain to see an invisible similitude in a visible one; and the use of images would make possible the "perfect possession" of a science and therewith the rapid comprehension of new things. Such a science might be teachable only in part to the soldiers of an army, that part being the art of war; but the art of war would imply a complete understanding and might therefore be said to encompass that understanding in a work addressed to actual princes in which Machiavelli does not advertise himself as the inventor of new modes and orders.[27] Even in *Discourses* III.39 Machiavelli speaks modestly of "general and particular knowledge" as knowledge of sites, and is far from claiming the glory of founding a new science in the comprehensive sense, as did Bacon and Descartes.

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[22] *The Prince* 6 (on armed prophets); 19 (on Severus); *Discourses*, Dedicatory Letter (on the writers' praise of Hiero); I.21; III.13, 38.

[23] Not in the *Art of War*, proemium, I (beg.), where Machiavelli says that the art of war is necessary to defend the other arts and is useful for civil life; not in his exchange with the French cardinal in *The Prince* 3, in which understanding war is distinct from understanding the state. See also the "arts of peace" in *Discourses* I.11.

[24] Cf. milder statements in *Discourses* I.21 and III.31.

[25] Livy merely says (XXXV.28.7) that Philopoemen did this so that "no consideration would be new to him in such a matter." Machiavelli has also suppressed Plutarch's remark that Philopoemen loved military affairs more than necessary; Plutarch, *Philopoemen* 4.6.

[26] For a more complete interpretation, see Mansfield (1979, pp. 421-24).

Machiavelli's "firm science," although making use of images, does not incur his own condemnation of the many (in *The Prince* 15) who have imagined republics and principalities "that have never been seen or known to exist in truth." Those imaginary states are based on a "profession of good," whereas Machiavelli's imagination begins from "what is done." In *Discourses* III.39 he reports Xenophon's use of hunting as an image of war, and by "hunting" Machiavelli indicates he means catching one's prey. One may suppose he was aware of the difference (and the similarity) between hunting as catching and hunting as dialectic in Plato's *Laws*.[28] If hunting is for catching, and hunting is an image of war, we can infer—because politics centers on acquisition—that in *The Prince* and the *Discourses* Machiavelli uses war as an image of politics. More attention might be given to the use of imagination in Machiavelli's behavioral political science.

Returning to chapter 14 of *The Prince,* we see that Machiavelli recommends for the second part of the art of war, the exercise of the mind, that the prince read histories in which the actions of excellent men are considered.[29] He must above all do as some excellent man has done in the past who has taken someone before him to imitate who had been praised and glorified, as it is said Alexander the Great imitated Achilles; Caesar, Alexander; Scipio, Cyrus. Thus, contrary to the impression given at the beginning of the sixth chapter, even excellent men may imitate others. Those they imitate are found in "histories," and as in *Discourses* III.39 Machiavelli singles out the "life of Cyrus written by Xenophon" and asserts that in his chastity, affability, humanity and liberality, Scipio conformed to what had been written about Cyrus by Xenophon. Those whom excellent men imitate, then, are men of whom authors such as Homer and especially Xenophon, the Socratic philosopher, have written. From whatever source princes learn the art of war, they imitate the virtues or qualities taught by authors, and, it is implied, their art is incomplete without these qualities. As with founders as Machiavelli describes them in *Discourses* I.9-10, princes possessing the art of war are not self-sufficient but dependent on the moral opinion of society. Accordingly, Machiavelli closes the chapter with a less promising remark about fortune; with this imitation a wise prince can *resist* fortune's adversities, not have a remedy for any accident.

With this modulation we are led to the fifteenth chapter of *The Prince,* entitled "On those things for which men and especially princes are praised or blamed." Here begins the second half of *The Prince,* in which the difficulties caused by the moral qualities are considered.[30] For apparently the art of war cannot teach one to surmount the moral expectations of one's subjects and friends which stand in the way of one's necessary acquisitions. Machiavelli's exaggeration of the art of war does put one in the right frame of mind; for in representing prudence as art, in contradiction to Aristotle, he makes prudence morally neutral (*Nicomachean Ethics* 1140b 25; 1144a 7-9, 23-37). We are thus prepared to be told that one must be prudent to know how to avoid the infamy of those vices that take away one's state (Ch. 15), for example, "the name of stingy" (Ch. 16), that a new prince must temper his cruelties with prudence and humanity (Ch. 17), and that a prudent lord should not keep faith when keeping it works against him (Ch. 18). Machiavelli offers a definition of prudence in chapter 21 that sums up its morally neutral use of the moral qualities: "Prudence consists in knowing how to recognize the qualities of inconveniences and in picking the less bad as good."

After chapter 14, meanwhile, the status of art is diminished as the arts are reduced to partial human activities such as the art of gaining reputation (Ch. 19) or subordinated to the virtues (Ch. 21) or contrasted with violence (Ch. 25). Remarkably, the art of war, which was said to be the sole object and thought of a prince in chapter 14 because it seemed to comprehend all human activities, is not even mentioned after that chapter. Instead, soldiers are said to love a prince of "military spirit" (Ch. 19), Caracalla and Philip of Macedon are praised as "military men" (Chs. 19, 24) and Lorenzo is exhorted to revive "military virtue" in Italy with a saying of Petrarch's predicting that virtue, not art, will take up arms there (Ch. 26).

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[27] In *Discourses* III.39, Decius, the Roman tribune, puts on a cloak so that the enemy would not notice the leader.

[28] Plato, *Laws* 823b; Xenophon makes a characteristically less obtrusive reference to "love of hunting" (*philotheria*), which Cyrus deprecates, *Cyropaideia* II.4.26.

[29] Histories, not mirrors of princes; cf. *Discourses* III.5.

[30] *Resta ora, The Prince* 15 begins; on the four parts of *The Prince,* see Strauss (1958, pp. 56-60).

As art is reduced to activities that can be ordered but are still subject to chance (Aristotle, *Nicomachean Ethics* 1140a 12-20), prudence is enlarged to include the governing of fortune. At the end of chapter 23, Machiavelli says that a prince who is not wise himself cannot be well advised unless by chance he gives himself over[31] to "one alone" (*uno solo*) to govern him in everything, who would be *uomo prudentissimo*. This second occurrence of *prudentissimo* in *The Prince*, used to describe the adviser of a prince who indeed governs him in everything, reminds us of the "very prudent" Paulo Vitelli (Ch. 12) who would have had the Florentines in a dilemma if he had taken Pisa. Machiavelli will show how to solve the problem of the adviser who governs the advised, thus benefiting the advised and, not incidentally, saving himself from the fate of Vitelli (cf. *Discourses* I.21, II.33). As adviser of princes (or republics), Machiavelli both is and is not a prince himself. He cannot acquire a new principality for himself, but in teaching princes he gains some of their glory, leaving for them the obvious glory enjoyed by successful politicians and taking for himself the glory of having facilitated their glory, a glory evident only to the discerning. Thus, in the next chapter[32] Machiavelli speaks of the double glory of "having given a beginning to a new principality" (*avere dato principio a uno principato nuovo*), a phrase that both falls short of and exceeds "acquiring a new principality." It exceeds acquiring a new principality because this prince does not merely imitate the great men whose beaten tracks a prudent man was previously required to follow (Ch. 6). His advice gives a beginning to every principality by supplying the moral prerequisites and their theoretical foundation.[33]

The last part of *The Prince* (Chs. 24-26) concerns the problem of sustaining the prince's glory despite "the brevity of life" (Ch. 25). Ordinary princes may not receive the glory they deserve or may receive more glory than they deserve, according to how ill or how well their habits and nature are suited to their times. But the prince who gave ordinary princes their beginning can afford to wait patiently while virtue and fortune are sorted out since the opportunities for his virtue, and hence his glory, are not bound by the brevity of his life.[34] He alone has sure defenses against stormy times, as only for him do such defenses depend on himself and his virtue (Ch. 24). Machiavelli's art has disappeared into his virtue, which is his alone. With his virtue he teaches the art of war or something like it to ordinary princes, but their opportunities for using his teaching depend on their fortune after all. Machiavelli will not allow "these princes of ours" to accuse fortune rather than admit their indolence; he demands, in the notorious sentence at the end of chapter 25, that they proceed impetuously to manhandle lady fortune. Imitating the furious Pope Julius II, they should go beyond where "all human prudence" would have led them. This now limited and subdued prudence serves to distinguish virtue from caution for the generality of Machiavelli's readers as they rush from the necessities he describes toward the prizes he holds out for them. But only he is truly impetuous. Only he is not bound by the moral qualities because only he has learned how to be good and not good. His impetuosity is both impetuous and respectful: impetuous in theory by comparison to other writers, and respectful in practice to Lorenzo and other princes, both actual and potential, to whom he allows their subordinate glories. His position as adviser enables him to share in the natures of all whom he advises as he uses the diverse qualities of those who follow his advice. So by the use of the moral qualities of others, he is not bound by his times as are others, hence not bound by his nature.[35] Since he is not bound by his nature and knows how to be flexible (both *facile* and *duro*) in all times,[36] his fortune is assured. Others may absorb his teaching and a few may perhaps equal or surpass him, but if his teaching is true they follow his fortune. They follow the fortune of the man who first showed men how to become responsible for their fortune. His virtue depends on himself alone, his glory is unrepeatable and he is *uno solo,* not in contemplative isolation but governing for the common benefit of each. His government is established *ad uno tratto* in his books, but also over time as his influence or fortune advances.

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[31] Machiavelli "gave himself over" to the orders of others (*The Prince* 13, end), took that back (15), and now looks for a prince who will give himself over to his adviser (23).

[32] *The Prince* 24 begins: "The things written above, observed prudently. . . ." On the glory of advising, see *Discourses* III.35.

[33] Cf. the double glory in *Discourses* III.13 and *Art of War* VII (in Machiavelli, 1961, p. 515).

[34] On brevity of life, see also *The Prince* 7, 11. And see *Discourses* III.8, 34, 35.

[35] "Nature" occurs 26 times in *The Prince* proper. The first occurrence is "the nature of government" (4); the ninth is "all other things in nature" (6); the eleventh is a "natural defect of spirit" (9); the thirteenth is "the nature of sites" (14); the fourteenth is "the nature of rivers and marshes" (14; cf. 25, beg.); the seventeenth is that a prince needs to know how to use "the one and the other nature" (18); and the nineteenth is "by nature or by art" (19). "Prudence" occurs 28 times and "art" 9 times. The sixth occurrence of "art" is with the nineteenth of "nature," the only association of "nature" and "art."

[36] See especially *The Prince* 24; *facile* and *duro* are the ninth pair in the list of eleven moral qualities in *The Prince* 15, distinct from the others as the qualities of qualities. See also *Discourses* I.41.

Machiavelli could be said to have "ornamented and confirmed" his new principality "with good laws, good arms, good friends and good examples" (Ch. 24) to be found in his books, but he did not write the handbook of an art of politics with rules for all occasions. Nor did he, like Thomas Hobbes, author a new political science or "civil philosophy" on the basis of a new method, and claim credit for it.[37] Even if Machiavelli had thought it possible for a political art to do the work of prudence, and even if the new scientific method had been available to him as it was to Hobbes, he would have had reason to decline the honor of founding a new political art or science. Such a founding would have given him too much glory because it would have subtracted from the glory of later princes—and not enough glory, because later princes applying an impersonal science would not have been subjects of his (see *Discourses* III.13, end).

The Cycle and the Soul

Machiavelli presents his new political science in opposition to classical political science. But despite his statement in chapter 15 of *The Prince* against the imaginary republics and principalities essential to classical political science, his opposition becomes clear only gradually. As can be seen from the preface to the first book of the *Discourses,* he found it convenient to condemn the weakness of his own age by contrast to the ancients, for in doing so he could appeal to opinion in his age favorable to the ancients. My account of his critique of classical political science, passing over the subtlety of his rhetoric, will consider two points of Machiavelli's attack, the cycle and the soul.

In the preface just mentioned, Machiavelli says that the ancients have been imitated in many things, though not in politics. Yet soon after, in *Discourses* I.2, he makes a significant departure from classical political science in regard to the cycle. Classical political science (here assembled from diverse sources) had supposed that regimes tend to change in a regular way—from good to less good to bad to worst; or alternating from good to bad and bad to good. These regular changes make a circle or cycle, coming back to their beginning and beginning again, with the consequence that the progress in human affairs that we moderns expect does not occur and should not be expected. Good times are followed by bad, and bad by good; so, as Isocrates once remarked, a reasonable man might wonder whether it is better to be born in good times that will worsen or bad times that will improve.[38] Civilization may progress as morality, arts and sciences advance; but civilizations are subject to natural catastrophes which return them to their barbarous beginnings, from which they must recommence. The cycle of regimes assumes that the city is self-sufficient; that it becomes better or worse through the actions of its own ruling class, which deserves praise or blame for them; that domestic policy is therefore primary. The cycle of civilizations, however, reminds men of the power of extra-human forces and of the fragility of human constructions. But this reminder of the limits to human choice actually promotes human choice because it teaches men that in the end all merely human force will be overborne, hence that they need not judge their actions merely by their consequences nor their governments by their durability. Moderation—not fearful, but responsible moderation—is the moral lesson of the classical teaching on the cycle.

Machiavelli did not approve of this lesson. In *Discourses* I.2 he almost copies an account of the cycle from Polybius, without naming him as source. The closeness of his copying enables one to see the significant differences, which center on his replacement of "nature" in Polybius by "chance" or "necessity" to explain the origin of governments and of morality.[39] After concluding this account, Machiavelli abruptly adds that almost no republic could survive these ups and downs without becoming subject to a neighboring state better ordered than itself. Speaking for himself ("I say"), he says that all these regimes are pestiferous, the good ones because of their "brevity of life" and the bad ones because of their malignity. Then, having introduced foreign policy as the decisive consideration and thus having denied the assumptions of the classical account of the cycle, he loses interest in the cycle of regimes and never discusses or even refers to it again.

Machiavelli offers, still in *Discourses* I.2, two kinds of legislated beginnings for cities: *uno solo* may make all the laws *ad uno tratto,* as Lycurgus in Sparta; or, as in Rome, the city may lack perfect order but may have made a good beginning apt to become better, and through the occurrence of accidents, may be capable of becoming perfect. At first the beginning which is legislated all at once—the mixed regime of classical political science—seems superior, but Machiavelli shows in *Discourses* I.3-8 that the way of perfection through accidents is far better. Although subject to party discord and "tumults" (in which men shout at each other instead of adjudicating their claims),[40] Rome's way was more flexible in meeting "accidents," especially the accident that if someone wishes to conquer you, you will have to conquer him first. Since every state must meet this accident, it should have a regime that enables it to keep what it has got, like Rome's. Rome could expand successfully, converting foreigners into Romans, while Sparta could not. Rome's regime of accidental perfection, with its emergency solutions and individual initiatives, was superior to Sparta's regime of planned perfection. One should not infer from this that human planning is incompetent in politics, however. We may suspect that Machiavelli, who had to overcome the prejudice against tumultuous republics (*Discourses* I.4), arranged the accidents of the Roman republic as supplied by Livy to suit his plan of perfection. His plan, we have suggested, is legislated all at once in his books but allows for accidents, especially of foreign policy, as they arise over time. It thus combines the two kinds of legislated beginnings set forth in *Discourses* I.2.

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[37] Hobbes (1839-45, I.viii-ix; VII.170-71). Cf. Sternberger (1974, pp. 40-41).

[38] Plato, *Laws* 676b-c; *Republic* 540e-541a; *Statesman* 271e-274e; Aristotle, *Physics* 223b 24-31; *Politics* 1316a 15-22; *On Philosophy* 8; Polybius, VI.9.10; Isocrates, *Areopagiticus* 3-5.

[39] Gennaro Sasso (1967, Chs. 4, 5; 1958, pp. 306-15). Strauss (1958, p. 222); Guillemain (1977, pp. 266-67).

One particular difficulty in the accidental perfection of Rome calls for Machiavelli's intervention. It appears in *Discourses* I.7, where he is considering why party government worked successfully in Rome and brought disaster in Florence. In Florence certain party politicians were able, when pressed, to appeal to "foreign forces," "outside forces," or "private forces." Machiavelli refers to Francesco Valori, a prince of the city as it were, a man who was judged to have wished "with his audacity and spiritedness [*animosità*] to rise above [*trascendere*] civil life," and who could be resisted only with a "sect" contrary to his. Valori's "sect" was the party of Savonarola, who is not mentioned. Soon after, in *Discourses* I. 11, he refers to Savonarola himself, and says that Savonarola was able to persuade the people of Florence, which did not think itself ignorant or rude, that he spoke with God. Clearly one who can persuade others that he speaks with God has a private advantage over other party politicians. He appeals to both a foreign (or outside) and a private source of power.

In the next chapter Machiavelli informs his readers that the Roman Church keeps Italy disunited, chiefly because the Church is not strong enough to seize Italy but strong enough to prevent anyone else from doing so. It prevents others from conquering—or should one say unifying?—Italy by appealing to outside powers such as the king of France to intervene on its behalf. But why can it do this? Because the pope can persuade not necessarily the king of France but the French people that he speaks with God. Thus Machiavelli speaks in this chapter of "Christian states and republics" and of the "Christian republic." Christian republics are divided from each other because of what unites them—a religion that gives opportunity, or makes it necessary, for priests to interfere in politics.

Therefore, a wider view is necessary. One must look not merely to the regime but also to the religion that controls the part of the world where the regime is, to the *sect*. "Sect" is an important Machiavellian term apparently borrowed from Marsilius; and Machiavelli follows Marsilius' impudent application of it to Christianity, for example in the strange phrase "Catholic sect."[41] Machiavelli takes up the cycle of sects in *Discourses* II.5, where he considers what cause is responsible for their rise and fall—men, heaven or nature. He raises the possibility that men, or even a single man, could control not the cycle of regimes but the cycle of sects. For sect is a more comprehensive phenomenon than regime; it includes language and customs, the moral climate of government, as well as politics. Even names and dates are determined by sects. In the *Florentine Histories* (I.5), Machiavelli refers to the fact that after Christianity was established, people stopped naming their sons Caesar and Pompey and began calling them Peter, John and Matthew. And he takes note of the manner in which we date events by ensuring that all the dates given in the *Discourses*—26 of them—occur in his own lifetime.

From the viewpoint of sects, one must consider foreign or outside forces in the widest sense of things foreign to or outside of man. One becomes aware that Machiavelli is not an unreflective humanist who puts human concerns first but a philosopher reflecting on God and nature who puts human necessities first. He does not behold God and nature with wonder, and discourse on what he understands of them. Rather, he regards them with fear, and sees this fear exemplified in fearful, wholesale changes of sect;[42] and so he discourses on the remedies he finds for the fears men must have of their beginnings. Then, from the viewpoint of sects, one could say that in modern times, that is, Machiavelli's times, the cycle is stalled. The proximate cause of the stall is Christianity, which does not esteem the honor of the world and keeps it weak (*Discourses* II.2). But the ultimate cause is in the principles of classical political science, perhaps especially in the classical notion of soul. In reading through *The Prince* and the *Discourses,* the only works of Machiavelli's in which he says he puts everything he knows, one does not find the word "soul" (*anima*).[43] What does this fact mean?

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[40] Aristotle, *Politics* 1280a 9-17.

[41] *Florentine Histories* I.5; V.1; Marsilius of Padua, *Defensor Pacis* I.X.3, 7; II.VIII.4; XVI.7; XX.1; Garin (1970, p. 61); Mansfield (1972, pp. 209-66).

[42] *Discourses* III.1; *Florentine Histories* I.5, VII.1.

As conceived by Plato and Aristotle, the soul was intended to give protection against "outside forces" in the extended sense. Having soul enables men to be different from their environment, to have dignity above the rest of nature and to be free of (or within) the forces of nature and gods outside them. In the classical definition of soul there were two essentials: the soul as beginning of motion and the soul as intellect. Having soul, men can begin an action on their own, not determined from outside; and because men have soul, this action can be intelligent. The problem of this definition was in connecting the two essentials. Whatever action one begins seems to be his own, and yet if the action is to be intelligent, one must be capable of detachment from one's own to achieve an impartial outlook that does not merely endorse one's prejudices. Yet while the two essentials are difficult to connect, it is also necessary to connect them. One's actions are not his own if they are determined by fancy or chance, that is, stupidly or by blindly following the authority of another.[44]

According to Machiavelli, and for a reason to be explained, there is terrible danger to mankind in the attempt to detach oneself from what is one's own. He therefore denied the possibility of detachment in the human soul. What was left in the classical definition was the soul as beginning of motion; but the soul cannot begin motion unless it can act intelligently. Other animals have instincts instead of intelligence and are incapable of truly voluntary or deliberate action. So, in denying the possibility of detached intelligence, Machiavelli had to pay the price of denying that men are capable of voluntary action.[45] But instead of having instincts, men are determined by necessity, or by the necessity that they have the prudence to recognize and foresee. Necessity well understood, not any fool's opinion, replaces deliberate choice in the soul as that which begins voluntary actions. Men may choose, but the only prudent choice is anticipation of necessity.[46] The human desire for glory, which seems to be opposed to necessity because it seems to seek what is in excess of necessary, is in truth comprised in necessity.[47] As we have seen, glory is redeemed from incaution by Machiavelli's plan for the common benefit of mankind, and at the same time it is required for the virtuous promotion of one's own.

It is notorious that Machiavelli once said in a letter that he loved his native country more than his own soul.[48] But which was his native country, Florence or Italy? Should he promote Florentine independence or Italian unity; and if one should get in the way of the other, which should he prefer? Machiavelli does not speak only to Florentines or to Italians but to all men. We may not be Florentines or Italians but we can nonetheless be Machiavellians. Machiavelli shows his solicitude for foreigners by giving them advice in both *The Prince* (3) and the *Discourses* (I.23) on how to invade Italy. Much as he loved Florence and Italy, he is not fundamentally a city or national patriot. He is a patriot on behalf of humanity, seeking to protect men against outside forces, consequently a patriot of the home of human beings, the earth.

In *Discourses* III.2 Machiavelli praises a trick played by Junius Brutus, the father of Roman liberty. According to the oracle of Apollo (as reported by Livy), the first among a group of young men, including Brutus, who kissed his mother, would come to the "highest power in Rome." Brutus decided that his mother was the earth, and pretended to fall in order to kiss the earth. Machiavelli urges us to believe not only that Brutus was ambitious for himself but also that he wished to crush the kings and liberate his native country. Summing up the point in its highest case, Machiavelli says of a "man who is notable for his quality" that he cannot live quietly and untroubled out of politics. However insistently he forswears honor and profit, his excuses will be heard but not accepted. The attitude of philosophic detachment is impossible even for one who does not feel the attraction of honor and profit. You cannot stand so far from princes as not to arouse their suspicion, and if you attempt it, you will nonetheless be involved in their fall. This "middle way" of classical political science between partisanship and unconcern cannot be sustained because the extreme of unconcern is impossible. Even Plato and Aristotle can be described (though to a pope) as no less ambitious for glory than are princes.[49]

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[43] Strauss (1958, pp. 200, 333n.59). *Anima* occurs occasionally in Machiavelli's other works.

[44] Plato, *Laws* 896a-e; *Phaedrus* 245c-d, 247d; *Timaeus* 89a; *Republic* 353d; *Phaedo* 105c; Aristotle, *De Anima* 404b 28-30, 432 a 15-18; *Nicomachean Ethics* 1139a 5-12.

[45] Men must make use of the beast, or beasts, in man; *The Prince* 18.

[46] *The Prince* 3, 6, 9, 25; *Discourses* I.1, 30, 32, 33, 52; III.5, 6.

[47] Thus *uno prudente e virtuoso* has the task of introducing form into the matter which will bring honor to him and good to the universality of men (*The Prince* 26); such a one is perhaps not *prudentissimo* to show so much of his virtue. This is the second of three instances of "form" and "matter" together in *The Prince,* where "form" appears 3 times and "matter" 13 times. See also *Discourses* I.1, 36, 37, 43, 60; III.12.

[48] Letter of April 16, 1527 to Vettori. See *Discursus florentinarum rerum post mortem iunioris Laurentii Medices,* in Machiavelli (1961, p. 275), and cf. Plato, *Laws* 731c. In his comments on Machiavelli's famous statement, Berlin (1980) makes it uncertain whether Machiavelli "revealed his basic moral beliefs" (p. 54) or left open the possibility of choosing to save one's soul (pp. 50, 64).

Necessity is always one's own necessity; that is why necessity overpowers any human capacity of detachment. What is necessary for me is especially what is necessary for me against you. Machiavelli says: "It is enough to ask someone for his weapon without saying, I wish to kill you with it; then after you have the weapon in your hand, you can satisfy your appetite" (*Discourses* I. 44). Since necessity is one's own as opposed to the necessity of others, one's designs prompted by necessity must be disguised. Like Brutus, the prudent man must use deceit. The most general mode of deception practiced by humans is by use of authority.[50] You say that some authority, for example the oracle of Apollo, supports or commands you, when in truth necessity requires it. When men use an authority, they put their own opinions in someone else's mouth—in the mouths of God or of their ancestors, or, like poets, in the mouths of their characters. Under necessity reason is translated into authority. Authority is reason in disguise, or better to say, authority is the effectual truth of detachment. Those who live a life of the mind, detached from politics, in effect elevate some authority in politics. This authority is not truly detached; it only pretends to be. It is their own necessity in disguise.

The greatest example of detachment become authority is the Christian God, which for Machiavelli, I will suggest, was the effectual truth of the classical notion of soul. The soul was intended to preserve human freedom and dignity, but the detachment of soul could be preserved only if one supposed that the soul is divine—that intelligence or soul is God, as Aristotle said. To prevent the divinity of soul from endangering human freedom, Aristotle may have conceived God as impersonal, but this reservation was unavailing. Any idea elevated above human beings is bound to be personified by them and made responsible for their good,[51] and the God of intelligence will be humanized and made providential. Aristotle's God was transformed into the personal Christian God that was used by priests to interfere with princes and tyrannize over peoples. The effectual truth of Aristotelianism was Christianity or that combination of Aristotle and Christianity which Thomas Hobbes was to call Aristotelity. Aristotle should have known that his detached intelligence in the soul would become a God that would be an outside force threatening the liberty of men.

Acting under necessity, then, Machiavelli substitutes what he calls *animo* ("spirit" or "spiritedness") for *anima* ("soul").[52] Whereas *anima* never occurs in *The Prince* and the *Discourses*, *animo* occurs frequently. *Animo* means a spirited defense of one's own, especially of one's own body; for *animo* defends a body and is satisfied with that body, but *anima* always attempts to transcend the body. *Animo* also means intent, as when Machiavelli urges the man notable for his quality to conceal his *animo* (*Discourses* III.2); but the intent, even in this case, is never contemplative or detached from one's own concerns. *Animo* is responsible for *ostinazione*, the obstinacy characteristic of spirited infantry who have planted their feet on the ground.[53]

Machiavelli's principle of spirited selfishness offers something for everybody, not in a common good to which all may contribute different virtues, but (as he says so precisely) for the common benefit of each one (*ciascuno*). Each one in his separate body is encouraged by Machiavelli in the spirited defense of his own, because one's own body is the only common thing that can be benefited. Thus everybody can fight for his own glory and yet be "good and faithful soldiers" to Machiavelli.[54] Machiavellianism can advance without paying allegiance to Machiavelli. A new sect will emerge with an invisible leader that will offer princes more glory and people more security. Not depending on the virtue of rulers but only on virtue in the Machiavellian sense of imaginative aggrandizement, this sect will not be subject to the ups and downs of the cycle. States will rise and fall, but the whole will remain strong and mankind will progress in a condition Machiavelli calls "the perpetual republic."[55]

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[49] "And this glory [of reforming republics and kingdoms] has been so esteemed by men who have not attended to anything other than glory that, when they have been unable to make a republic in deed, they have made it in writing, like Aristotle, Plato and many others. They wished to show the world that if they were unable to found a civil association like Solon and Lycurgus, they did not fail because of their ignorance but because of their impotence. . ." (Machiavelli, *Discursus*, 1961, pp. 275-76). See also Manent (1977, p. 21).

[50] *The Prince* 7; *Discourses* I.7, 9, 34, 35, 53; II.12, 18; III.3, 5, 10, 30, 35, 38, 48.

[51] As Machiavelli personifies *fortuna*, *The Prince* 25; *Discourses* II.29. See *Discourses* I.53 for an instance of the people being deceived by a "false image of good"; also I.56, III.43. On the cause of making the Church powerful, see *The Prince* 3 (end), 11.

[52] As in the striking phrase, *virtù di corpo e di animo*, *The Prince* 8; *Discourses* III.8; *Florentine Histories* VI.6; and in *grande virtù di animo*, *The Prince* 19; *grandezza dello anima*, 8, 26. See also the greater reliability of touching than seeing, *The Prince* 18 (end), and the use of *umori* (humors of the body) in *The Prince* 9, 19; *Discourses* I.4. Scholarly discussion of *virtù* in Machiavelli as a rule forgets to ask what it is that virtue is the virtue of.

[53] There is republican obstinacy (*Discourses* II.2), religious obstinacy (I.14, 15), obstinacy in the infantry (II.16, 17; III.12), and obstinate conspirators (III.6; *The Prince* 19).

If Machiavelli's ambition seems grandiose, we should look carefully at the modern world we live in to see whether our ambition, which resembles his, is reasonable. Machiavelli, indeed, left a restraint on human aggrandizement which has not proved durable. Since for him there is no soul or principle *above* one's own country, there could be no universal principle *beyond* one's country. The crucial political implication is that, for Machiavelli, no *patria* could ever regard itself as essential to the destiny of mankind. There could be no chosen people, race or class. Machiavelli would have abhorred our twentieth-century tyrannies, but with our century in view he might have to admit that he had left no effectual alternative to dubious patriotism but a modern version of the "pious cruelty" (*The Prince* 21) he had meant to destroy. Twentieth-century totalitarianism promises fantastic betterment in the human condition from the realization of universal principles, but it does not claim to improve the soul. It has, without reference to soul, proved to exceed the pious cruelty of any sect with which Machiavelli was acquainted.

Perhaps, then, not the asserted existence of soul but loss of moderation in the soul has been the cause of our troubles. But we shall never learn whether this is so if we hold to a political science that routinely excludes soul from what it calls "behavior" and flutters at the mention of virtue.

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[54] *Discourses* I.43. See *The Prince* 19 (end) on maintaining a state without having the respect of peoples.

[55] A perpetual republic is denied in *Discourses* III.17, but affirmed in III.22 after the remedy is found; and it is assumed possible in I.20; II.5; III.1, 3.

The Emergence of Cooperation among Egoists

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This article investigates the conditions under which cooperation will emerge in a world of egoists without central authority. This problem plays an important role in such diverse fields as political philosophy, international politics, and economic and social exchange. The problem is formalized as an iterated Prisoner's Dilemma with pairwise interaction among a population of individuals.

Results from three approaches are reported: the tournament approach, the ecological approach, and the evolutionary approach. The evolutionary approach is the most general since all possible strategies can be taken into account. A series of theorems is presented which show: (1) the conditions under which no strategy can do any better than the population average if the others are using the reciprocal cooperation strategy of TIT FOR TAT, *(2) the necessary and sufficient conditions for a strategy to be collectively stable, and (3) how cooperation can emerge from a small cluster of discriminating individuals even when everyone else is using a strategy of unconditional defection.*

Under what conditions will cooperation emerge in a world of egoists without central authority? This question has played an important role in a variety of domains including political philosophy, international politics, and economic and social exchange. This article provides new results which show more completely than was previously possible the conditions under which cooperation will emerge. The results are more complete in two ways. First, *all* possible strategies are taken into account, not simply some arbitrarily selected subset. Second, not only are equilibrium conditions established, but also a mechanism is specified which can move a population from noncooperative to cooperative equilibrium.

The situation to be analyzed is the one in which narrow self-maximization behavior by each person leads to a poor outcome for all. This is the famous Prisoner's Dilemma game. Two individuals can each either cooperate or defect. No matter what the other does, defection yields a higher payoff than cooperation. But if both defect, both do worse than if both cooperated. Figure 1 shows the payoff matrix with sample utility numbers attached to the payoffs. If the other player cooperates, there is a choice between cooperation which yields R (the reward for mutual cooperation) or defection which yields T (the temptation to defect). By assumption, $T > R$, so it pays to defect if the other player cooperates. On the other hand, if the other player defects, there is a choice between cooperation which yields S (the sucker's payoff), or defection which yields P (the punishment for mutual defection). By assumption, $P > S$, so it pays to defect if the other player defects. Thus no matter what the other player does, it pays to defect. But if both defect, both get P rather than the R they could both have got if both had cooperated. But R is assumed to be greater than P. Hence the dilemma. Individual rationality leads to a worse outcome for both than is possible.

To insure that an even chance of exploitation or being exploited is not as good an outcome as mutual cooperation, a final inequality is added in the standard definition of the Prisoner's Dilemma. This is just $R > (T+S)/2$.

Thus two egoists playing the game once will both choose their dominant choice, defection, and get a payoff, P, which is worse for both than the R they could have got if they had both cooperated. If the game is played a known finite number of times, the players still have no incentive to

	Cooperate	Defect
Cooperate	R=3, R=3	S=0, T=5
Defect	T=5, S=0	P=1, P=1

$$T > R > P > S$$
$$R > (S+T)/2$$

Source: Robert Axelrod, "Effective Choice in the Prisoner's Dilemma," *Journal of Conflict Resolution* 24 (1980): 3-25; "More Effective Choice in the Prisoner's Dilemma," *Journal of Conflict Resolution* 24 (1980): 379-403.

Note: The payoffs to the row chooser are listed first.

Figure 1. A Prisoner's Dilemma

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cooperate. This is certainly true on the last move since there is no future to influence. On the next-to-last move they will also have no incentive to cooperate since they can anticipate mutual defection on the last move. This line of reasoning implies that the game will unravel all the way back to mutual defection on the first move of any sequence of plays which is of known finite length (Luce and Raiffa, 1957, pp. 94-102). This reasoning does not apply if the players will interact an indefinite number of times. With an indefinite number of interactions, cooperation can emerge. This article will explore the precise conditions necessary for this to happen.

The importance of this problem is indicated by a brief explanation of the role it has played in a variety of fields.

1. *Political Philosophy.* Hobbes regarded the state of nature as equivalent to what we now call a two-person Prisoner's Dilemma, and he built his justification for the state upon the purported impossibility of sustained cooperation in such a situation (Taylor, 1976, pp. 98-116). A demonstration that mutual cooperation could emerge among rational egoists playing the iterated Prisoner's Dilemma would provide a powerful argument that the role of the state should not be as universal as some have argued.

2. *International Politics.* Today nations interact without central control, and therefore the conclusions about the requirements for the emergence of cooperation have empirical relevance to many central issues of international politics. Examples include many varieties of the security dilemma (Jervis, 1978) such as arms competition and its obverse, disarmament (Rapoport, 1960); alliance competition (Snyder, 1971); and communal conflict in Cyprus (Lumsden, 1973). The selection of the American response to the Soviet invasion of Afghanistan in 1979 illustrates the problem of choosing an effective strategy in the context of a continuing relationship. Had the United States been perceived as continuing business as usual, the Soviet Union might have been encouraged to try other forms of noncooperative behavior later. On the other hand, any substantial lessening of U.S. cooperation risked some form of retaliation which could then set off counter-retaliation, setting up a pattern of mutual defection that could be difficult to get out of. Much of the domestic debate over foreign policy is over problems of just this type.

3. *Economic and social exchange.* Our everyday lives contain many exchanges whose terms are not enforced by any central authority. Even in economic exchanges, business ethics are maintained by the knowledge that future interactions are likely to be affected by the outcome of the current exchange.

4. *International political economy.* Multinational corporations can play off host governments to lessen their tax burdens in the absence of coordinated fiscal policies between the affected governments. Thus the commodity exporting country and the commodity importing country are in an iterated Prisoner's Dilemma with each other, whether they fully appreciate it or not (Laver, 1977).

In the literatures of these areas, there has been a convergence on the nature of the problem to be analyzed. All agree that the two-person Prisoner's Dilemma captures an important part of the strategic interaction. All agree that what makes the emergence of cooperation possible is the possibility that interaction will continue. The tools of the analysis have been surprisingly similar, with game theory serving to structure the enterprise.

As a paradigm case of the emergence of cooperation, consider the development of the norms of a legislative body, such as the United States Senate. Each senator has an incentive to appear effective for his or her constituents even at the expense of conflicting with other senators who are trying to appear effective for *their* constituents. But this is hardly a zero-sum game since there are many opportunities for mutually rewarding activities between two senators. One of the consequences is that an elaborate set of norms, or folkways, have emerged in the Senate. Among the most important of these is the norm of reciprocity, a folkway which involves helping out a colleague and getting repaid in kind. It includes vote trading, but it extends to so many types of mutually rewarding behavior that "it is not an exaggeration to say that reciprocity is a way of life in the Senate" (Matthews, 1960, p. 100; see also Mayhew, 1974).

Washington was not always like this. Early observers saw the members of the Washington community as quite unscrupulous, unreliable, and characterized by "falsehood, deceit, treachery" (Smith, 1906, p. 190). But by now the practice of reciprocity is well established. Even the significant changes in the Senate over the last two decades toward more decentralization, more openness, and more equal distribution of power have come without abating the folkway of reciprocity (Ornstein, Peabody and Rhode, 1977). I will show that we do *not* need to assume that senators are more honest, more generous, or more public-spirited than in earlier years to explain how cooperation based on reciprocity has emerged and proven stable. The emergence of cooperation can be explained as a consequence of senators pursuing their own interests.

The approach taken here is to investigate how individuals pursuing their own interests will act, and then see what effects this will have for the system as a whole. Put another way, the approach is to make some assumptions about micro-motives, and then deduce consequences for macro-behavior (Schelling, 1978). Thinking about the paradigm case of a legislature is a convenience, but the same style of reasoning can apply to the emergence of cooperation between individuals in many other political settings, or even to relations between nations. While investigating the conditions which foster the emergence of cooperation, one should bear in mind that cooperation is not always socially desirable. There are times when public policy is best served by the prevention of cooperation—as in the need for regulatory action to prevent collusion between oligopolistic business enterprises.

The basic situation I will analyze involves pairwise interactions.[1] I assume that the player can recognize another player and remember how the two of them have interacted so far. This allows the history of the particular interaction to be taken into account by a player's strategy.

A variety of ways to resolve the dilemma of the Prisoner's Dilemma have been developed. Each involves allowing some additional activity which alters the strategic interaction in such a way as to fundamentally change the nature of the problem. The original problem remains, however, because there are many situations in which these remedies are not available. I wish to consider the problem in its fundamental form.

1. There is *no mechanism available to the players to make enforceable threats or commitments* (Schelling, 1960). Since the players cannot make commitments, they must take into account all possible strategies which might be used by the other player, and they have all possible strategies available to themselves.

2. There is no way to be sure what the other player will do on a given move. This eliminates the possibility of metagame analysis (Howard, 1971) which allows such options as "make the same choice as the other player is about to make." It also eliminates the possibility of reliable reputations such as might be based on watching the other player interact with third parties.

3. There is no way to change the other player's utilities. The utilities already include whatever consideration each player has for the interests of the other (Taylor, 1976, pp. 69-83).

Under these conditions, words not backed by actions are so cheap as to be meaningless. The players can communicate with each other only through the sequence of their own behavior. This is the problem of the iterated Prisoner's Dilemma in its fundamental form.

Two things remain to be specified: how the payoff of a particular move relates to the payoff in a whole sequence, and the precise meaning of a strategy. A natural way to aggregate payoffs over time is to assume that later payoffs are worth less than earlier ones, and that this relationship is expressed as a constant discount per move (Shubik, 1959, 1970). Thus the next payoff is worth only a fraction, w, of the same payoff this move. A whole string of mutual defection would then have a "present value" of $P + wP + w^2P + w^3P \ldots = P/(1-w)$. The discount parameter, w, can be given either of two interpretations. The standard economic interpretation is that later consumption is not valued as much as earlier consumption. An alternative interpretation is that future moves may not actually occur, since the interaction between a pair of players has only a certain probability of continuing for another move. In either interpretation, or a combination of the two, w is strictly between zero and one. The smaller w is, the less important later moves are relative to earlier ones.

For a concrete example, suppose one player is following the policy of always defecting, and the other player is following the policy of *TIT FOR TAT*. *TIT FOR TAT* is the policy of cooperating on the first move and then doing whatever the other player did on the previous move. This means that *TIT FOR TAT* will defect once for each defection by the other player. When the other player is using *TIT FOR TAT*, a player who always defects will get T on the first move, and P on all the subsequent moves. The payoff to someone using *ALL D* when playing with someone using *TIT FOR TAT* is thus:

$$\begin{aligned} V(ALL\,D \mid TFT) &= T + wP + w^2P + w^3P \ldots \\ &= T + wP(1 + w + w^2 \ldots) \\ &= T + wP/(1-w). \end{aligned}$$

Both *ALL D* and *TIT FOR TAT* are strategies. In general, a *strategy* (or decision rule) is a function from the history of the game so far into a probability of cooperation on the next move. Strategies can be stochastic, as in the example of a rule which is entirely random with equal probabilities of cooperation and defection on each move. A strategy can also be quite sophisticated in its use of the pattern of outcomes in the game so far to determine what to do next. It may, for example, use Bayesian techniques to estimate the parameters of some model it might have of the other player's rule. Or it may be some complex combination of other strategies. But a strategy must rely solely on the information available through this one sequence of interactions with the particular player.

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[1] A single player may be interacting with many others, but the player is interacting with them one at a time. The situations which involve more than pairwise interaction can be modeled with the more complex n-person Prisoner's Dilemma (Olson, 1965; G. Hardin, 1968; R. Hardin, 1971; Schelling, 1973). The principal application is to the provision of collective goods. It is possible that the results from pairwise interactions will help suggest how to undertake a deeper analysis of the n-person case as well, but that must wait. For a parallel treatment of the two-person and n-person cases, see Taylor (1976, pp. 29-62).

The first question one is tempted to ask is, "What is the best strategy?" This is a good question, but unfortunately there is no best rule independent of the environment which it might have to face. The reason is that what works best when the other player is unconditionally cooperative will not in general work well when the other player is conditionally cooperative, and vice versa. To prove this, I will introduce the concept of a *nice* strategy, namely, one which will never be the first to defect.

THEOREM 1. *If the discount parameter,* w, *is sufficiently high, there is no best strategy independent of the strategy used by the other player.*

PROOF. Suppose A is a strategy which is best regardless of the strategy used by the other player. This means that for any strategies A' and B, $V(A|B) \geqslant V(A'|B)$. Consider separately the cases where A is nice and A is not nice. If A is nice, let $A' = ALL\ D$, and let $B = ALL\ C$. Then $V(A|B) = R/(1-w)$ which is less than $V(A'|B) = T/(1-w)$. On the other hand, if A is not nice, let $A' = ALL\ C$, and let B be the strategy of cooperating until the other player defects and then always defects. Eventually A will be the first to defect, say, on move n. The value of n is irrelevant for the comparison to follow, so assume that $n = 1$. To give A the maximum advantage, assume that A always defects after its first defection. Then $V(A|B) = T + wP/(1-w)$. But $V(A'|B) = R/(1-w) = R+wR/(1-w)$. Thus $V(A|B) < V(A'|B)$ whenever $w > (T-R)/(T-P)$. Thus the immediate advantage gained by the defection of A will eventually be more than compensated for by the long-term disadvantage of B's unending defection, assuming that w is sufficiently large. Thus, if w is sufficiently large, there is no one best strategy.

In the paradigm case of a legislature, this theorem says that if there is a large enough chance that a member of Congress will interact *again* with another member of Congress, then there is no one best strategy to use independently of the strategy being used by the other person. It would be best to be cooperative with someone who will reciprocate that cooperation in the future, but not with someone whose future behavior will not be very much affected by this interaction (see, for example, Hinckley, 1972). The very possibility of achieving stable mutual cooperation depends upon there being a good chance of a continuing interaction, as measured by the magnitude of w. Empirically, the chance of two members of Congress having a continuing interaction has increased dramatically as the biennial turnover rates in Congress have fallen from about 40 percent in the first 40 years of the Republic to about 20 percent or less in recent years (Young, 1966, pp. 87-90; Jones, 1977, p. 254; Patterson, 1978, pp. 143-44). That the increasing institutionalization of Congress has had its effects on the development of congressional norms has been widely accepted (Polsby, 1968, esp. n. 68). We now see how the diminished turnover rate (which is one aspect of institutionalization) can allow the development of reciprocity (which is one important part of congressional folkways).

But saying that a continuing chance of interaction is necessary for the development of cooperation is not the same as saying that it is sufficient. The demonstration that there is not a single best strategy still leaves open the question of what patterns of behavior can be expected to emerge when there actually is a sufficiently high probability of continuing interaction between two people.

The Tournament Approach

Just because there is no single best decision rule does not mean analysis is hopeless. For example, progress can be made on the question of which strategy does best in an environment of players who are also using strategies designed to do well. To explore this question, I conducted a tournament of strategies submitted by game theorists in economics, psychology, sociology, political science and mathematics (Axelrod, 1980a). Announcing the payoff matrix shown in Figure 1, and a game length of 200 moves, I ran the 14 entries and *RANDOM* against each other in a round robin tournament. The result was that the highest average score was attained by the simplest of all the strategies submitted, *TIT FOR TAT*.

I then circulated the report of the results and solicited entries for a second round. This time I received 62 entries from six countries.[2] Most of the contestants were computer hobbyists, but there were also professors of evolutionary biology, physics, and computer science, as well as the five disciplines represented in the first round. *TIT FOR TAT* was again submitted by the winner of the first round, Anatol Rapoport of the Institute for Advanced Study (Vienna). And it won again. An analysis of the 3,000,000 choices which were made in the second round shows that *TIT FOR TAT* was a very robust rule because it was nice, provocable into a retaliation by a defection of the other, and yet forgiving after it took its one retaliation (Axelrod, 1980b).

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[2] In the second round, the length of the games was uncertain, with an expected median length of 200 moves. This was achieved by setting the probability that a given move would not be the last one at $w = .99654$. As in the first round, each pair was matched in five games. See Axelrod (1980b) for a complete description.

The Ecological Approach

To see if *TIT FOR TAT* would do well in a whole series of simulated tournaments, I calculated what would happen if each of the strategies in the second round were submitted to a hypothetical next round in proportion to its success in the previous round. This process was then repeated to generate the time path of the distribution of strategies. The results showed that as the less-successful rules were displaced, *TIT FOR TAT* continued to do well with the rules which initially scored near the top. In the long run, *TIT FOR TAT* displaced all the other rules and went to what biologists call fixation (Axelrod, 1980b).

This is an ecological approach because it takes as given the varieties which are present and investigates how they do over time when interacting with each other. It provides further evidence of the robust nature of the success of *TIT FOR TAT*.

The Evolutionary Approach

A much more general approach would be to allow all possible decision rules to be considered, and to ask what are the characteristics of the decision rules which are stable in the long run. An evolutionary approach recently introduced by biologists offers a key concept which makes such an analysis tractable (Maynard Smith, 1974, 1978). This approach imagines the existence of a whole population of individuals employing a certain strategy, *B*, and a single mutant individual employing another strategy, *A*. Strategy *A* is said to *invade* strategy *B* if $V(A|B) > V(B|B)$ where $V(A|B)$ is the expected payoff an *A* gets when playing a *B*, and $V(B|B)$ is the expected payoff a *B* gets when playing another *B*. Since the *B*'s are interacting virtually entirely with other *B*'s, the concept of invasion is equivalent to the single mutant individual being able to do better than the population average. This leads directly to the key concept of the evolutionary approach. A strategy is *collectively stable* if no strategy can invade it.[3]

The biological motivation for this approach is based on the interpretation of the payoffs in terms of fitness (survival and fecundity). All mutations are possible, and if any could invade a given population it would have had the chance to do so. Thus only a collectively stable strategy is expected to be able to maintain itself in the long-run equilibrium as the strategy used by all.[4] Collectively stable strategies are important because they are the only ones which an entire population can maintain in the long run if mutations are introduced one at a time.

The political motivation for this approach is based on the assumption that all strategies are possible, and that if there were a strategy which would benefit an individual, someone is sure to try it. Thus only a collectively stable strategy can maintain itself as the strategy used by all—provided that the individuals who are trying out novel strategies do not interact too much with one another.[5] As we shall see later, if they do interact in clusters, then new and very important developments become possible.

A difficulty in the use of this concept of collective stability is that it can be very hard actually to determine which strategies have it and which do not. In most biological applications, including both the Prisoner's Dilemma and other types of interactions, this difficulty has been dealt with in one of two ways. One method has been to restrict the analysis to situations where the strategies take some particularly simple form such as in one-parameter models of sex-ratios (Hamilton, 1967). The other method has been to restrict the strategies themselves to a relatively narrow set so that some illustrative results could be attained (Maynard Smith and Price, 1973; Maynard Smith, 1978).

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[3] Those familiar with the concepts of game theory will recognize this as a strategy being in Nash equilibrium with itself. My definitions of invasion and collective stability are slightly different from Maynard Smith's (1974) definitions of invasion and evolutionary stability. His definition of invasion allows $V(A|B) = V(B|B)$ provided that $V(B|A) > V(A|A)$. I have used the new definitions to simplify the proofs and to highlight the difference between the effect of a single mutant and the effect of a small number of mutants. Any rule which is evolutionarily stable is also collectively stable. For a nice rule, the definitions are equivalent. All theorems in the text remain true if "evolutionary stability" is substituted for "collective stability" with the exception of Theorem 3, where the characterization is necessary but no longer sufficient.

[4] For the application of the results of this article to biological contexts, see Axelrod and Hamilton (1981). For the development in biology of the concepts of reciprocity and stable strategy, see Hamilton (1964), Trivers (1971), Maynard Smith (1974), and Dawkins (1976).

[5] Collective stability can also be interpreted in terms of a commitment by one player, rather than the stability of a whole population. Suppose player *Y* is committed to using strategy *B*. Then player *X* can do no better than use this same strategy *B* if and only if strategy *B* is collectively stable.

The difficulty of dealing with all possible strategies was also faced by Michael Taylor (1976), a political scientist who sought a deeper understanding of the issues raised by Hobbes and Hume concerning whether people in a state of nature would be expected to cooperate with each other. He too employed the method of using a narrow set of strategies to attain some illustrative results. Taylor restricted himself to the investigation of four particular strategies (including *ALL D, ALL C, TIT FOR TAT,* and the rule "cooperate until the other defects, and then always defect"), and one set of strategies which retaliates in progressively increasing numbers of defections for each defection by the other. He successfully developed the equilibrium conditions when these are the only rules which are possible.[6]

Before running the computer tournament I believed that it was impossible to make very much progress if all possible strategies were permitted to enter the analysis. However, once I had attained sufficient experience with a wide variety of specific decision rules, and with the analysis of what happened when these rules interacted, I was able to formulate and answer some important questions about what would happen if all possible strategies were taken into account.

The remainder of this article will be devoted to answering the following specific questions about the emergence of cooperation in the iterated Prisoner's Dilemma:

1. Under what conditions is *TIT FOR TAT* collectively stable?
2. What are the necessary and sufficient conditions for any strategy to be collectively stable?
3. If virtually everyone is following a strategy of unconditional defection, when can cooperation emerge from a small cluster of newcomers who introduce cooperation based on reciprocity?

TIT FOR TAT as a Collectively Stable Strategy

TIT FOR TAT cooperates on the first move, and then does whatever the other player did on the previous move. This means that any rule which starts off with a defection will get T, the highest possible payoff, on the first move when playing *TIT FOR TAT*. Consequently, *TIT FOR TAT* can only avoid being invadable by such a rule if the game is likely to last long enough for the retaliation to counteract the temptation to defect. In fact, no rule can invade *TIT FOR TAT* if the discount parameter, w, is sufficiently large. This is the heart of the formal result contained in the following theorem. Readers who wish to skip the proofs can do so without loss of continuity.

THEOREM 2. TIT FOR TAT *is a collectively stable strategy if and only if* $w \geq \max\left(\frac{T-R}{T-P}, \frac{T-R}{R-S}\right)$. *An alternative formulation of the same result is that* TIT FOR TAT *is a collectively stable strategy if and only if it is invadable neither by* ALL D *nor the strategy which alternates defection and cooperation.*

PROOF. First we prove that the two formulations of the theorem are equivalent, and then we prove both implications of the second formulation. To say that *ALL D* cannot invade *TIT FOR TAT* means that $V(ALL\ D|TFT) \leq V(TFT|TFT)$. As shown earlier, $V(ALL\ D|TFT) = T + wP/(1-w)$. Since *TFT* always cooperates with its twin, $V(TFT|TFT) = R + wR + w^2R\ldots = R/(1-w)$. Thus *ALL D* cannot invade *TIT FOR TAT* when $T + wP/(1-w) \leq R/(1-w)$, or $T(1-w) + wP \leq R$, or $T-R \leq w(T-P)$ or $w \geq \frac{T-R}{T-P}$. Similarly, to say that alternation of D and C cannot invade *TIT FOR TAT* means that $(T+wS)/(1-w^2) \leq R/(1-w)$, or $\frac{T-R}{R-S} \leq w$. Thus $w \geq \frac{T-R}{T-P}$ and $w \geq \frac{T-R}{R-S}$ is equivalent to saying that *TIT FOR TAT* is invadable by neither *ALL D* nor the strategy which alternates defection and cooperation. This shows that the two formulations are equivalent.

Now we prove both of the implications of the second formulation. One implication is established by the simple observation that if *TIT FOR TAT* is a collectively stable strategy, then no rule can invade, and hence neither can the two specified rules. The other implication to be proved is that if neither *ALL D* nor Alternation of D and C can invade *TIT FOR TAT*, then no strategy can.

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[6] For the comparable equilibrium conditions when all possible strategies are allowed, see below, Theorems 2 through 6. For related results on the potential stability of cooperative behavior, see Luce and Raiffa (1957, p. 102), Kurz (1977) and Hirshleifer (1978).

TIT FOR TAT has only two states, depending on what the other player did the previous move (on the first move it assumes, in effect, that the other player has just cooperated). Thus if *A* is interacting with *TIT FOR TAT,* the best which any strategy, *A,* can do after choosing *D* is to choose *C* or *D.* Similarly, the best *A* can do after choosing *D* is to choose *C* or *D.* This leaves four possibilities for the best *A* can do with *TIT FOR TAT:* repeated sequences of *CC, CD, DC,* or *DD.* The first does the same as *TIT FOR TAT* does with another *TIT FOR TAT.* The second cannot do better than both the first and the third. This implies if the third and fourth possibilities cannot invade *TIT FOR TAT,* then no strategy can. These two are equivalent, respectively, to Alternation of *D* and *C,* and *ALL D.* Thus if neither of these two can invade *TIT FOR TAT,* no rule can, and *TIT FOR TAT* is a collectively stable strategy.

The significance of this theorem is that it demonstrates that if everyone in a population is cooperating with everyone else because each is using the *TIT FOR TAT* strategy, no one can do better using any other strategy *provided* the discount parameter is high enough. For example, using the numerical values of the payoff parameters given in Figure 1, *TIT FOR TAT* is uninvadable when the discount parameter, *w,* is greater than 2/3. If *w* falls below this critical value, and everyone else is using *TIT FOR TAT,* it will pay to defect on alternative moves. For *w* less than 1/2, *ALL D* can also invade.

One specific implication is that if the other player is unlikely to be around much longer because of apparent weakness, then the perceived value of *w* falls and the reciprocity of *TIT FOR TAT* is no longer stable. We have Caesar's explanation of why Pompey's allies stopped cooperating with him. "They regarded his [Pompey's] prospects as hopeless and acted according to the common rule by which a man's friends become his enemies in adversity" (trans. by Warner, 1960, p. 328). Another example is the business institution of the factor who buys a client's accounts receivable. This is done at a very substantial discount when the firm is in trouble because

> once a manufacturer begins to go under, even his best customers begin refusing payment for merchandise, claiming defects in quality, failure to meet specifications, tardy delivery, or what-have-you. The great enforcer of morality in commerce is the continuing relationship, the belief that one will have to do business again with this customer, or this supplier, and when a failing company loses this automatic enforcer, not even a strong-arm factor is likely to find a substitute (Mayer, 1974, p. 280).

Similarly, any member of Congres who is perceived as likely to be defeated in the next election may have some difficulty doing legislative business with colleagues on the usual basis of trust and good credit.[7]

There are many other examples of the importance of long-term interaction for the stability of cooperation in the iterated Prisoner's Dilemma. It is easy to maintain the norms of reciprocity in a stable small town or ethnic neighborhood. Conversely, a visiting professor is likely to receive poor treatment by other faculty members compared to the way these same prople treat their regular colleagues.

Another consequence of the previous theorem is that if one wants to prevent rather than promote cooperation, one should keep the same individuals from interacting too regularly with each other. Consider the practice of the government selecting two aerospace companies for competitive development contracts. Since firms specialize to some degree in either air force or in navy planes, there is a tendency for firms with the same specialties to be frequently paired in the final competition (Art, 1968). To make tacit collusion between companies more difficult, the government should seek methods of compensating for the specialization. Pairs of companies which shared a specialization would then expect to interact less often in the final competitions. This would cause the later interactions between them to be worth relatively less than before, reducing the value of *w.* If *w* is sufficiently low, reciprocal cooperation in the form of tacit collusion ceases to be a stable policy.

Knowing when *TIT FOR TAT* cannot be invaded is valuable, but it is only a part of the story. Other strategies may be, and in fact are, also collectively stable. This suggests the question of what a strategy has to do to be collectively stable. In other words, what policies, if adopted by everyone, will prevent any one individual from benefiting by a departure from the common strategy?

The Characterization of Collectively Stable Strategies

The characterization of all collectively stable strategies is based on the idea that invasion can be prevented if the rule can make the potential invader worse off than if it had just followed the common strategy. Rule B can prevent invasion by rule A if B can be sure that no matter what A does later, B will hold A's total score low enough. This leads to the following useful definition: B has a *secure position* over A on move n if no matter what A does from move n onwards, $V(A|B) \leqslant V(B|B)$, assuming that B defects from move n onwards. Let $V_n(A|B)$ represent A's discounted cumulative score in the moves before move n. Then another way of saying that B has a secure position over A on move n is that

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[7] A countervailing consideration is that a legislator in electoral trouble may receive help from friendly colleagues who wish to increase the chances of reelection of someone who has proven in the past to be cooperative, trustworthy, and effective. Two current examples are Morris Udall and Thomas Foley. (I wish to thank an anonymous reviewer for this point.)

$$V_n(A|B) + w^{n-1}P/(1-w) \leqslant V(B|B),$$

since the best A can do from move n onwards if B defects is get P each time. Moving the second term to the right side of the inequality gives the helpful result that B has a secure position over A on move n if $V_n(A|B)$ is small enough, namely if

$$V_n(A|B) \leqslant V(B|B)-w^{n-1}P/(1-w). \tag{1}$$

The theorem which follows embodies the advice that if you want to employ a collectively stable strategy, you should only cooperate when you can afford an exploitation by the other side and still retain your secure position.

THEOREM 3. THE CHARACTERIZATION THEOREM. B *is a collectively stable strategy if and only if* B *defects on move* n *whenever the other player's cumulative score so far is too great, specifically when* $V_n(A|B) > V(B|B) - w^{n-1}(T+wP/(1-w))$.

PROOF. First it will be shown that a strategy B which defects as required will always have a secure position over any A, and therefore will have $V(A|B) \leqslant V(B|B)$ which in turn makes B a collectively stable strategy. The proof works by induction. For B to have a secure position on move 1 means that $V(A|ALL\ D) \leqslant V(ALL\ D|ALL\ D)$ according to the definition of secure position applied to n=1. Since this is true for all A, B has a secure position on move 1. If B has a secure position over A on move n, it has a secure position on move n+1. This is shown in two parts.

First, if B defects on move n, A gets at most P, so

$$V_{n+1}(A|B) \leqslant V_n(A|B) + w^{n-1}P.$$

Using Equation (1) gives:

$$V_{n+1}(A|B) \leqslant V(B|B) - w^{n-1}P/(1-w) + w^{n-1}P$$

$$V_{n+1}(A|B) \leqslant V(B|B) - w^{n}P/(1-w).$$

Second, B will only cooperate on move n when

$$V_n(A|B) \leqslant V(B|B) - w^{n-1}(T+wP/(1-w)).$$

Since A can get at most T on move n, we have

$$V_{n+1}(A|B) \leqslant V(B|B) - w^{n-1}(T+wP/(1-w)) + w^{n-1}T$$

$$V_{n+1}(A|B) \leqslant V(B|B) - w^{n}P/(1-w).$$

Therefore, B always has a secure position over A, and consequently B is a collectively stable strategy.

The second part of the proof operates by contradiction. Suppose that B is a collectively stable strategy and there is an A and an n such that B does not defect on move n when

$$V_n(A|B) > V(B|B) - w^{n-1}(T+wP/(1-w)),$$

i.e., when

$$V_n(A|B) + w^{n-1}(T+wP/(1-w)) > V(B|B). \tag{2}$$

Define A' as the same as A on the first $n-1$ moves, and D thereafter. A' gets T on move n (since B cooperated then), and at least P thereafter. So,

$$V(A'|B) \geqslant V_n(A|B) + w^{n-1}(T+wP/(1-w)).$$

Combined with (2) this gives $V(A'|B) > V(B|B)$. Hence A' invades B, contrary to the assumption that B is a collectively stable strategy. Therefore, if B is a collectively stable strategy, it must defect when required.

Figure 2 illustrates the use of this theorem. The dotted line shows the value which must not be exceeded by any A if B is to be a collectively stable strategy. This value is just $V(B|B)$, the expected payoff attained by a player using B when virtually all the other players are using B as well. The solid curve represents the critical value of A's cumulative payoff so far. The theorem simply says that B is a collectively stable strategy if and only if it defects whenever the other player's cumulative value so far in the game is above this line. By doing so, *B* is able to prevent the other player from eventually getting a total expected value of more than rule *B* gets when playing another rule *B*.

The characterization theorem is "policy-relevant" in the abstract sense that it specifies what a strategy, *B*, has to do at any point in time as a function of the previous history of the interaction in order for *B* to be a collectively stable strategy.[8] It is a complete characterization because this requirement is both a necessary and a sufficient condition for strategy *B* to be collectively stable.

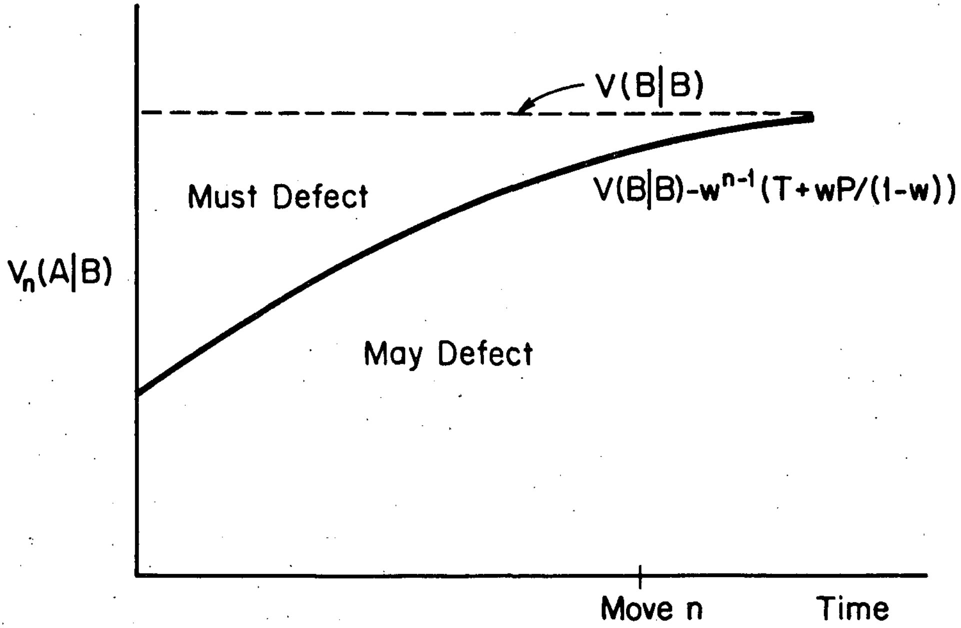
Two additional consequences about collectively stable strategies can be seen from Figure 2. First, as long as the other player has not accumulated too great a score, a strategy has the flexibility to either cooperate or defect and still be collectively stable. This flexibility explains why there are typically many strategies which are collectively stable. The second consequence is that a nice rule (one which will never defect first) has the most flexibility since it has the highest possible score when playing an identical rule. Put another way, nice rules can afford to be more generous than other rules with potential invaders because nice rules do so well with each other.

The flexibility of a nice rule is not unlimited, however, as shown by the following theorem. In fact, a nice rule must be *provoked* by the very first defection of the other player, i.e., on some later move the rule must have a finite chance of retaliating with a defection of its own.

THEOREM 4. *For a nice strategy to be collectively stable, it must be provoked by the first defection of the other player.*

PROOF. If a nice strategy were not provoked by a defection on move *n*, then it would not be collectively stable because it could be invaded by a rule which defected only on move *n*.

Besides provocability, there is another requirement for a nice rule to be collectively stable. This requirement is that the discount parameter *w*, be sufficiently large. This is a generalization of the second theorem, which showed that for *TIT FOR TAT* to be collectively stable, *w* has to be large enough. The idea extends beyond just nice rules to any rule which might be the first to cooperate.



Source: Drawn by the author.

Figure 2. Characterization of a Collectively Stable Strategy

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[8] To be precise, $V(B|B)$ must also be specified in advance. For example, if *B* is never the first to defect, $V(B|B) = R/(1-w)$.

THEOREM 5. *Any rule,* B, *which may be the first to cooperate is collectively stable only when* w *is sufficiently large.*

PROOF. If B cooperates on the first move, $V(ALL\ D|B) \geqslant T + wP/(1-w)$. But for any B, $R/(1-w) \geqslant V(B|B)$ since R is the best B can do with another B by the assumptions that $R>P$ and $R>(S+T)/2$. Therefore $V(ALL\ D|B) > V(B|B)$ is so whenever $T + wP/(1-w) > R/(1-w)$. This implies that $ALL\ D$ invades a B which cooperates on the first move whenever $w<\frac{T-R}{T-P}$. If B has a positive chance of cooperating on the first move, then the gain of $V(ALL\ D|B)$ over $V_1(B|B)$ can only be nullified if w is sufficiently large. Likewise, if B will not be the first to cooperate until move n, $V_n(ALL\ D|B) = V_n(B|B)$ and the gain of $V_{n+1}(ALL\ D|B)$ over $V_{n+1}(B|B)$ can only be nullified if w is sufficiently large.

There is one strategy which is *always* collectively stable, that is regardless of the value of w or the payoff parameters T, R, P, and S. This is *ALL D,* the rule which defects no matter what.

THEOREM 6. ALL D *is always collectively stable.*

PROOF. *ALL D* is always collectively stable because it always defects and hence it defects whenever required by the condition of the characterization theorem.

This is an important theorem because of its implications for the evolution of cooperation. If we imagine a system starting with individuals who cannot be enticed to cooperate, the collective stability of *ALL D* implies that no single individual can hope to do any better than just to go along and be uncooperative as well. A world of "meanies" can resist invasion by anyone using any other strategy—provided that the newcomers arrive one at a time.

The problem, of course, is that a single newcomer in such a mean world has no one who will reciprocate any cooperation. If the newcomers arrive in small clusters, however, they will have a chance to thrive. The next section shows how this can happen.

The Implications of Clustering

To consider arrival in clusters rather than singly, we need to broaden the idea of "invasion" to include invasion by a cluster.[9] As before, we will suppose that strategy B is being used by virtually everyone. But now suppose that a small group of individuals using strategy A arrives and interacts with both the other A's and the native B's. To be specific, suppose that the proportion of the interactions by someone using strategy A with another individual using strategy A is p. Assuming that the A's are rare relative to the B's, virtually all the interactions of B's are with other B's. Then the average score of someone using A is $pV(A|A) + (1-p)V(A|B)$ and the average score of someone using B is $V(B|B)$. Therefore, a *p-cluster of* A *invades* B if $pV(A|A) + (1-p)V(A|B) > V(B|B)$, where p is the proportion of the interactions by a player using strategy A with another such player. Solving for p, this means that invasion is possible if the newcomers interact enough with each other, namely when

$$p > \frac{V(B|B)-V(A|B)}{V(A|A)-V(A|B)}. \tag{3}$$

Notice that this assumes that pairing in the interactions is not random. With random pairing, an A would rarely meet another A. Instead, the clustering concept treats the case in which the A's are a trivial part of the environment of the B's, but a nontrivial part of the environment of the other A's.

The striking thing is just how easy invasion of *ALL D* by clusters can be. Specifically, the value of p which is required for invasion by *TIT FOR TAT* of a world of *ALL D*'s is surprisingly low. For example, suppose the payoff values are those of Figure 1, and that $w = .9$, which corresponds to a 10-percent chance two interacting players will never meet again. Let A be *TIT FOR TAT* and B be *ALL D.* Then $V(B|B) = P/(1-w) = 10$; $V(A|B) = S + wP/(1-w) = 9$; and $V(A|A) = R/(1-w) = 30$. Plugging these numbers into Equation (3) shows that a p-cluster of *TIT FOR TAT* invades *ALL D* when $p>1/21$. Thus if the newcomers using *TIT FOR TAT* have any more than about 5 percent of their interactions with others using *TIT FOR TAT,* they can thrive in a world in which everyone else refuses ever to cooperate.

In this way, a world of meanies can be invaded by a cluster of *TIT FOR TAT*—and rather easily at that. To illustrate this point, suppose a business school teacher taught a class to give cooperative behavior a chance, and to reciprocate cooperation from other firms. If the students did, and if they did not disperse too widely (so that a sufficient proportion of their interactions were with others from the same class), then the students would find that their lessons paid off.

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[9] For related concepts from biology, see Wilson (1979) and Axelrod and Hamilton (1981).

When the interactions are expected to be of longer duration (or the time discount factor is not as great), then even less clustering is necessary. For example, if the median game length is 200 moves (corresponding to $w = .99654$) and the payoff parameters are as given in Figure 1, even one interaction out of a thousand with a like-minded follower of *TIT FOR TAT* is enough for the strategy to invade a world of *ALL D*'s. Even when the median game length is only two moves ($w = .5$), anything over a fifth of the interactions by the *TIT FOR TAT* players with like-minded types is sufficient for invasion to succeed and cooperation to emerge.

The next result shows which strategies are the most efficient at invading *ALL D* with the least amount of clustering. These are the strategies which are best able to discriminate between themselves and *ALL D*. A strategy is *maximally discriminating* if it will eventually cooperate even if the other has never cooperated yet, and once it cooperates it will never cooperate again with *ALL D* but will always cooperate with another player using the same strategy.

THEOREM 7. *The strategies which can invade* ALL D *in a cluster with the smallest value of* p *are those which are maximally discriminating, such as* TIT FOR TAT.

PROOF. To be able to invade *ALL D,* a rule must have a positive chance of cooperating first. Stochastic cooperation is not as good as deterministic cooperation with another player using the same rule since stochastic cooperation yields equal probability of S and T, and $(S+T)/2<R$ in the Prisoner's Dilemma. Therefore, a strategy which can invade with the smallest p must cooperate first on some move, n, even if the other player has never cooperated yet. Employing Equation (3) shows that the rules which invade B=*ALL D* with the lowest value of p are those which have the lowest value of p^*, where $p^* = [V(B|B) - V(A|B)]/[V(A|A) - V(A|B)]$. The value of p^* is minimized when $V(A|A)$ and $V(A|B)$ are maximized (subject to the constraint that A cooperates for the first time on move n) since $V(A|A) > V(B|B) > V(A|B)$. $V(A|A)$ and $V(A|B)$ are maximized subject to this constraint if and only if A is a maximally discriminating rule. (Incidentally, it does not matter for the minimal value of p just when A starts to cooperate.) *TIT FOR TAT* is such a strategy because it always cooperates for n=1, it cooperates only once with *ALL D,* and it always cooperates with another *TIT FOR TAT.*

The final theorem demonstrates that nice rules (those which never defect first) are actually better able than other rules to protect themselves from invasion by a cluster.

THEOREM 8. *If a nice strategy cannot be invaded by a single individual, it cannot be invaded by any cluster of individuals either.*

PROOF. For a cluster of rule A to invade a population of rule B, there must be a $p\leqslant 1$ such that $pV(A|A) + (1-p)V(A|B) > V(B|B)$. But if B is nice, then $V(A|A) \leqslant V(B|B)$. This is so because $V(B|B) = R/(1-w)$, which is the largest value attainable when the other player is using the same strategy. It is the largest value since $R>(S+T)/2$. Since $V(A|A) \leqslant V(B|B)$, A can invade as a cluster only if $V(A|B) > V(B|B)$. But that is equivalent to A invading as an individual.

This shows that nice rules do not have the structural weakness displayed in *ALL D. ALL D* can withstand invasion by any strategy, as long as the players using other strategies come one at a time. But if they come in clusters (even in rather small clusters), *ALL D* can be invaded. With nice rules, the situation is different. If a nice rule can resist invation by other rules coming one at a time, then it can resist invasion by clusters, no matter how large. So nice rules can protect themselves in a way that *ALL D* cannot.[10]

In the illustrative case of the Senate, Theorem 8 demonstrates that once cooperation based on reciprocity has become established, it can remain stable even if a cluster of newcomers does not respect this senatorial folkway. And Theorem 6 has shown that without clustering (or some comparable mechanism) the original pattern of mutual "treachery" could not have been overcome. Perhaps these critical early clusters were based on the boardinghouse arrangements in the capital during the Jeffersonian era (Young, 1966). Or perhaps the state delegations and state party delegations were more critical (Bogue and Marlaire, 1975). But now that the pattern of reciprocity is established, Theorems 2 and 5 show that it is collectively stable, as long as the biennial turnover rate is not too great.

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[10] This property is possessed by population mixes of nice rules as well. If no single individual can invade a population of nice rules, no cluster can either.

Thus cooperation can emerge even in a world of unconditional defection. The development cannot take place if it is tried only by scattered individuals who have no chance to interact with each other. But cooperation can emerge from small clusters of discriminating individuals, as long as these individuals have even a small proportion of their interactions with each other. Moreover, if nice strategies (those which are never the first to defect) eventually come to be adopted by virtually everyone, then those individuals can afford to be generous in dealing with any others. The population of nice rules can also protect themselves against clusters of individuals using any other strategy just as well as they can protect themselves against single individuals. But for a nice strategy to be stable in the collective sense, it must be provocable. So mutual cooperation can emerge in a world of egoists without central control, by starting with a cluster of individuals who rely on reciprocity.

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Political and Mathematical Perspectives on Educational Equity

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A selective review of the literature on equal educational opportunity supports an argument for explicit use of normative models in research. The argument has two parts. First, we show that confusion over the Coleman Report (1966) was due to a mismatch between the implied reform objective and the statistical approach. The objective was to reduce black/white differences in educational achievement, but the statistics explained individual differences in achievement. Second, we partly develop and apply a normative dynamic model of the objectives of school integration over time. Normative dynamic models could be used in conjunction with behavioral data to operationalize theoretical concepts such as accountability, symbolic politics and non-decisions.

Critics say that social scientists fail to make values explicit in their research (Meehan, 1973; Storing, 1962; Walker, 1966). The first section of this article illustrates the problem with a discussion of the Coleman Report (1966). We emphasize that the contrasting findings result from differences in normative/mathematical frameworks, not from differences in data or raw statistics, such as means, standard deviations, or regression coefficients. Indeed, the contrasting findings that we present in Tables 1 and 2 below are generated by the same regression equations.[1] One of the most disturbing features of the Coleman Report episode is the failure of peer review. Despite extensive reanalysis and discussion, critics generally did not attribute the report's counter-intuitive findings to a mismatch between reforms under consideration (to eliminate the black/white achievement gap), and the statistical approach used in the analysis (to explain individual differences in achievement).

The second section of the article partially develops and applies a normative, dynamic model of reform expectations for integration, as of 1966. The model is intended to operationalize political constraints and the effects of desegregation on social relationships over time. At the close of the section, we touch on possible applications of this sort of model in policy analysis, program evaluation, implementation and basic research. From a theoretical perspective, the normative model might be used with behavioral data to operationalize Edelman's concept of symbolic politics (1971, 1977) and Bachrach's and Baraetz's concept of non-decisions (1962, 1963). Also, because the reform model predicts changes in the rules of the game governing "who gets what," as these are reflected in regression coefficients, it does not have the conservative implications that are often attributed to strictly empirical analyses of equity issues (Aaron, 1978, p. 98).

The following symbols will be used throughout:

A = Achievement as measured by conventional tests;
S = School quality indicators;
C = Class or social background;
M = Merit or genuine talent;
p = Raw regression coefficients for the black population;
q = Raw regression coefficients for the white population;
b = Black;
w = White;
i = Individual;
t = Time.

The author is a Program Officer at the National Science Foundation. However, all opinions and conclusions are those of the author and do not reflect the views of NSF.

[1] Speaking of the data presented in Table 1, McPartland and Sprehe say (1973, pp. 323): "The variables for these regression analyses are the same variables which were used in the major regression analyses of the 1966 report." They list these variables by grade. Coleman (1972, p. 160), who produced Table 2 after the report, says that it employs measures of the sort used in the report scaled up, and he lists the number of variables that apply in each of four categories. The variable numbers in Coleman's categories are the same as those reported by McPartland and Sprehe. Of course, both tables use the same data. Thus, while there may be modest differences in the equations that undergird the two tables, they are substantially the same. The contrast in findings in the tables stems from the different weights that are applied to the raw regression coefficients. This is explained at length in the text.

The Coleman Episode, or What Was the Question?

Three normative perspectives on educational equity are especially useful in understanding the controversy generated by the study of equal educational opportunity conducted by Coleman and his colleagues (1966). These are the objectives of (1) equality of schools for blacks and whites, (2) equality of average achievement for blacks and whites, and (3) equality of individual achievement. Each of these objectives is defined statistically in this section. (See Miller, 1977, 1981, for graphic representations.)

We show that the confusion generated by the Coleman study stems from the fact that it was widely thought to address the first two policy objectives, but actually it addressed the first and third. Misapplied as an explanation of achievement differences between the races, explanations of individual differences within each race contradict the conventional wisdom about the importance and effects of schools. In fact, schools explain a substantial portion of the black/white achievement gap, and only a trivial portion of individual achievement differences.

It must be stressed again, that contrasts in the explanations of between-race and within-race differences in achievement do not reflect contradictory findings of fact. The data, variance, variables and form of the equation can all be held constant, while explanations diverge, owing to the mathematical frameworks that are used to interpret the raw statistics. These mathematical frameworks, in turn, embody contrasting assumptions, values, and policy perspectives.

Equal Schools for Blacks and Whites. The normative goal is to eliminate past and present manifestations of racial discrimination so that schools for the minority will be equal in quality to schools for the majority. Some variance in school quality is expected within each race, but the reform goal requires that on the average schools be equal. The end result should be $\overline{S}_w = \overline{S}_b$ for the various S_1, $S_2, \ldots S_n$ that are significant indicators of school quality. Graphically, the problem is that the distribution (and mean) of school resources for blacks is at a lower level than that for whites. Equalization requires merging the two distributions, which could be achieved by raising resources for blacks, lowering resources for whites, or some combination of the two.

There are at least two views on what constitutes school equality. In breaking with the tradition of "separate but equal" schools, the 1954 *Brown* decision established the principle that segregated schools are "inherently" unequal. From this perspective, Coleman's study found extensive school inequality—that is, racial segregation. Most American school children were attending schools where children of their own color constituted a large proportion of the student body (National Institute of Education, 1976, pp. 23-24).

On the other hand, to the surprise of Coleman and others, the data showed that school spending and the material qualities of the schools attended by blacks and whites were quite similar. Mosteller and Moynihan say (1972, p. 11):

> The nation had, within geographic regions, come close to supplying equal, though largely separate, facilities to Negroes and whites according to the classical notion of traditional physical inputs. Insofar as that is true, the country, having chosen a goal in a social problem, had come close to reaching it.

The report also found that the remaining differences between the schools, including segregation, had modest effects on achievement. It is this latter finding that needs to be put in the context of the normative framework that produced it.

Equal Achievement for Blacks and Whites. The Coleman Report (1966) is credited with changing the operational definition of "equal educational opportunity." Before the report, the objective "was measured in terms of school inputs, including racial mixture"; after the report "it became increasingly the practice, even the demand, that equality be measured by school outputs; that is to say, by results of tests of academic achievement" (Mosteller and Moynihan, 1972, p. 6). In the words of analysts for the National Institute of Education (1976, pp. 5-6), "Better educational opportunities for blacks became associated with closing the educational achievement gap between blacks and whites." The graphic statement of the equal average achievement objective is the same as for the equal school objective, but the variable is achievement, and the equity goal is $\overline{A}_w = \overline{A}_b$, or more realistically, given constraints, to minimize the difference in average attainment—Min $(\overline{A}_w - \overline{A}_b)$. Again, equalization can be achieved by raising black achievement levels, lowering white achievement levels, or a combination of both.

Coleman's original analysis plan (1972, pp. 153-55) was to explain lower achievement levels for blacks using equations that related achievement to race-linked differences in school characteristics, controlling for other factors. Regression coefficients in an equation which related A (achievement) to indicators of S (school quality) for the black population could then be used as weights to estimate the effects on black achievement of replacing the "levels of input resources for the average black in the region with those of the white in the same region," while holding C (home background) at black averages. This analysis produces two types of findings. First, it provides a measure of "effective inequality of opportunity": the average gain in black achievement resulting from exposure to all school resources enjoyed by whites. Second, by selectively changing some school variables while keeping others at average levels, the researcher can identify school equalization policies that are expected to raise black achievement most rapidly.

Operationally, then, the measure of unequal opportunity resulting from black/white school differences would be $\hat{A}_{b,t+n} - \hat{A}_{b,t}$, where $\hat{A}_{b,t+n}$ is the estimated achievement of blacks after a completed reform (t+n) that exposes them to the school advantages enjoyed by the average white. The proposed measure of effective inequality of opportunity is equal to the sum of the products of the school variable raw regression coefficients in the black equation ($p_{as.c}$) and the average black/white school variable differences:[2]

$$\hat{A}_{b,t+n} - \bar{A}_{b,t} = \sum_{s=1}^{n} p_{as.c}(\bar{S}_{w,t} - \bar{S}_{b,t}).$$

Also, the $p_{as.c}$ $(\bar{S}_w - \bar{S}_b)$ that makes the largest contribution in explaining effective inequality of opportunity would be the key target for equalization.

Coleman (1972, p. 155) did not carry out the analysis as planned. McPartland and Sprehe (1973, p. 323) did execute Coleman's original concept using the same data and variables that were employed in the major regressions of the 1966 report. Consistent with the unconstrained objective stated above to minimize the difference between black and white achievement by any means—gains for blacks, or losses for whites—McPartland and Sprehe calcuated impacts for both races. Thus in addition to the equations estimating potential gains for the average black illustrated above, equations estimate potential losses to the average white from exposure to conditions faced by the average black:

$$\hat{A}_{w,t+n} - \bar{A}_{w,t} = \sum_{s=1}^{n} q_{as.c}\ (\bar{S}_{w,t} - \bar{S}_{b,t}).$$

Results of the McPartland and Sprehe reanalysis are presented below in terms of standardized units of achievement. Figures in the body of the table are changes in achievement that are predicted to result from black exposure to school characteristics experienced by the average white in the same region, and from white exposure to school characteristics experienced by the average black.[3]

The predicted changes in achievement indicate that inequalities between schools are important in explaining the achievement gap between blacks and whites. Changes in achievement resulting from school equalization are greatest for whites in the sixth and ninth grades. For example, the .61 loss of a standard unit of achievement predicted for sixth-grade southern whites who attend typical schools for southern blacks accounted for 57 percent of the gap in racial achievement in that region and grade.[4] If southern whites went to the "separate but equal" schools of blacks, the estimates say that their achievement advantage over southern blacks would have been half as great as it was in the mid-1960s. The table also tells us that the white advantage in achievement is largely attributable to differences in teacher characteristics.

Another striking feature of the estimates is the contrast between the sensitivity of whites and blacks to school characteristics in the sixth and ninth grades. Whites are generally more sensitive to changes. In contrast to the largest loss for whites (.79 of a standard unit of achievement), the largest gain in predicted achievement for blacks receiving white school inputs is .33 of a standard unit, or only 24 percent of the twelfth-grade racial achievement gap in the South. While the generalization from the sixth and ninth grades is contradicted by a reversal in the twelfth grade in this table, results for the sixth and ninth grades are believable because they are consistent with the literature, which shows that raw regression coefficients calculated on white populations are generally higher than those calculated for blacks, when the dependent variable is achievement or attainment (Miller, 1981, with supporting evidence from Jensen, 1968, pp. 59-66; 1972, p. 161; Jencks, 1979, pp. 192-212; Brown and Saks, 1975, p. 592; Featherman and Hauser, 1978, pp. 313-84; Heyns, 1976). This is true regardless of the input variables. Thus, generally, $p_{as.c} < q_{as.c}$.

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[2] Operationalization of this approach to produce results found in Tables 1 and 3 would require an additional step. The products would be standardized by the standard deviation of the achievement variable.

[3] For example, the figure .61 in the top left of the table means that the expected change in average white student achievement owing to a simulated "reassignment" of whites to typical black schools is a drop of 61 percent of one standard deviation of achievement. Note that the response of blacks in the same grade and region appears to be of smaller magnitude. Since the absolute difference in inputs, $|\bar{S}_w - \bar{S}_b|$, that is multiplied by each regression coefficient is the same in both estimates, the primary explanation for the larger change predicted for whites is larger, (on average) raw regression coefficients in the white equation. In fact, in all but the twelfth grade northern sample, the standard deviations used to standardize the sum of the products are larger in the white samples. So these figures in Table 1 make the difference between raw regression coefficients that generally favors whites ($p < q$) appear to be smaller than it actually is.

[4] The percentage of the achievement gap that is explained is calculated by dividing standardized achievement changes in the table by the difference between black/white means in the appropriate region and grade, after standardizing that difference by dividing it by the standard deviation of achievement within the group for which the equation was estimated. The means and variance figures for achievement are from Coleman's statistical appendix (1966).

In contrast to McPartland and Sprehe (1973), who explained average differences in achievement between the races in terms of average differences in school quality, Coleman's report (1966) explained individual differences in achievement in terms of individual exposure to school and home advantages. The normative political perspective that corresponds most closely to the Coleman analytic approach is, as we shall see in the next sub-section, the equalization of individual differences in achievement. The large discrepancies in the findings from the between-race and individual modes of analysis is evident when one compares a summary table prepared by Coleman (1972, p. 160) with the table above. Figures in the cells of Table 2 are measures of the unique contribution of school factors and family factors to variance in individual achievement.

Although variables and equations for the two tables are the same (see n. 1), the explanation of achievement provided in Table 2 contrasts with that in Table 1. Table 2 indicates that school characteristics explain very little of the variance in

Table 1. Inequalities Due to Race

Subgroups Equation Used	School Resources Assigned	Change in Expected Achievement Due to Reassignment of:			
		Facilities	Teachers	Student Body	Total School Resources
White South	BS				
6		−.01	−.59	−.01	−.61
9		.00	−.70	−.08	−.79
12		.01	−.13	−.12	−.25
Average of three grades		.00	−.47	−.07	−.55
White North	BN				
6		−.04	−.29	−.10	−.43
9		.00	−.30	−.11	−.40
12		.03	−.13	−.08	−.18
Average of three grades		.00	−.24	−.10	−.34
Black South	WS				
6		−.02	.18	.09	.25
9		−.03	.14	.18	.29
12		.00	.16	.17	.33
Average of three grades		−.02	.16	.15	.29
Black North	WN				
6		.02	.04	.13	.19
9		.06	.02	.11	.19
12		.00	.10	.15	.25
Average of three grades		.03	.05	.13	.21

Source: The table is taken directly from James McPartland and J. Timothy Sprehe, "Racial and Regional Inequalities in School Resources Relative to Their Educational Outcomes," *Social Science Research* 2 (1973), 321-32. It is their reanalysis of data from James S. Coleman, *Equality of Educational Opportunity* (Washington, D.C.: U.S. Government Printing Office, 1966).

Key: BS = Black South
BN = Black North
WS = White South
WN = White North

Table 2. Inequalities Due to Individual Opportunities within Categories of Race and Region

	Black North	Black South	White North	White South
Grade 12				
Family	.067	.119	.133	.144
Facilities and curriculum	.018	.009	.014	.007
Teacher	.016	.026	.009	.002
Student body	.046	.078	.008	.013
R^2	.146	.232	.165	.166
Grade 9				
Family	.065	.098	.160	.183
Facilities and curriculum	.017	.046	.010	.004
Teacher	.012	.014	.010	.010
Student body	.027	.053	.006	.005
R^2	.121	.211	.186	.203
Grade 6				
Family	.086	.137	.135	.179
Facilities and curriculum	.002	.033	.004	.003
Teacher	.024	.023	.007	.011
Student body	.021	.021	.017	.006
R^2	.132	.213	.163	.199

Source: This table is taken directly from James S. Coleman, "The Evaluation of Equality of Educational Opportunity," in Frederick Mosteller and Daniel P. Moynihan (eds.), *On Equality of Educational Opportunity* (New York: Vintage Books, 1972). Only the title is changed from the original. The original title was "Unique contributions to variance in verbal achievement (scaled up to sum to R^2 in each regression) as measures of the importance of each of four clusters of variables: family background (6 variables), school facilities and curriculum (11 variables), teacher characteristics (7 variables), and student-body characteristics (5 variables), blacks and whites in North and South, Grades 12, 9, 6."

achievement, and teachers do not stand out as having the greatest influence on white student achievement. Also in contrast with Table 1, school variables explain more of the variation in achievement for blacks than for whites. This finding in Table 2 makes it appear that blacks are more sensitive to school variables, as Coleman reported (1966, p. 317). However, raw regression coefficients are better indicators of sensitivity than are measures of variance explained (Hanushek and Kain, 1972, pp. 135-36), and we can infer from Table 1 that the regression coefficients are generally larger for whites than they are for blacks (see n. 3).

If the raw statistics—regresssion coefficients, means, standard deviations—behind the two tables are the same, what accounts for the different explanations of achievement? As noted above, the tables provide answers to two different questions. The McPartland and Sprehe analysis asks and answers questions about the causes of achievement differences between the races. The Coleman analysis asks and answers questions about individual achievement differences within the races. Both studies address a common total distribution of individual differences, but the first focuses on the relatively small between-race portion of that distribution and the second focuses on the relatively large within-race portion.

Although the studies are logically and statistically compatible, the reader who thought that both analyses explained achievement in an absolute sense would conclude that social scientists had come up with conflicting findings. One study found that schools had substantial effects, while the other found they had inconsequential effects; one study found that schools were more important for whites, while the other found schools more important for blacks; and one study found that teacher quality was the most important school factor for whites, while the other found other school factors equally important. But when these findings are placed in the context of their normative/statistical frameworks, they do not conflict. Rather, the first study explains the achievement gap between the faces, while the other explains achievement differences among individuals within each race.

Equal Achievement for Individuals. A little over five years after Coleman redefined equal educational opportunity, Christopher Jencks and his colleagues (1972) tried to convince readers to accept yet another substitute for equalizing schools.

Jencks, who must be credited for supplying explicit linkages between values and statistical methods, warns readers that he is not addressing the widely accepted objective of reducing racial disparities in achievement (1972, p. 14):

> The reader should also be warned that we are primarily concerned with inequality between individuals, not inequality between groups. This accounts for much of the discrepancy between our conclusions and those of others who have examined the same data. There is always far more inequality between individuals than between groups. It follows that when we compare the degree of inequality between groups to the degree of inequality between individuals, inequality between groups often seems relatively unimportant. It seems quite shocking, for example, that white workers earn 50 percent more than black workers. But we are even more disturbed by the fact that the best-paid fifth of all white workers earns 600 percent more than the worst-paid fifth. From this viewpoint, racial inequality looks almost insignificant.

Stated statistically, the objective of equalizing individual differences in achievement is to minimize the variance or average sum of the squares of individual achievement around the mean: Min $(A_i - \bar{A}_i)^2/N$ or $Min\ \delta_i^2$. This has the property of placing priority on eliminating the "extremes of wealth and poverty," which is Jencks' political objective (1972, p. 14). In other words, the reform objective is $\delta^2_{i,t+n} < \delta^2_{i,t}$. Graphically, this implies squeezing the distribution of individual differences in achievement so that individuals are more like one another.

Although Coleman (1966) did not embrace the reduction of individual differences in achievement as his policy objective, his statistical approach does so—it is the same as that of Jencks, and explains individual differences in achievement. It is this mismatch between Coleman's apparent articulation of a between-race policy objective and his statistical analysis of within-race variation that is responsible for the confusion over the report. To be fair, however, Coleman and his colleagues never inappropriately claim that they are explaining black/white differences. Readers, not the authors, leap from within-race findings to between-race conclusions.

Why Findings Differ: Value-Laden Assumptions. We have presented two tables that are compatible if readers understand that Table 1 explains achievement differences between the races (in terms of average school quality differences between the races), while Table 2 explains individual achievement differences (in terms of individual exposure to school and background advantages within the races). Different value-relevant assumptions characterize the two statistical approaches and we explore some of these. The nature of the implied normative assumptions illustrates further that it is inappropriate to generalize from studies of individuals to groups.

In defining what is to be explained, the framework for studying individuals makes no distinction between variation in achievement that is independent of social background (and likely to result from merit) and variation in achievement that is associated with race (and likely to result from racial discrimination). Indeed, in order to find the best predictors of individual achievement, rigorous analyses of individual differences (such as Table 2) focus on achievement variation among students of the same social background, and ask schools to explain all differences that are left after statistical adjustments that eliminate (control for) differences owing to race, region and home background. Average school quality indicators account for very little of the variation in achievement that is independent of demographic characteristics, which is to be expected, since the bulk of it lies among family members and individuals who attend the same schools (Mosteller and Moynihan, 1972, pp. 17-18).

In the framework for studying racial differences, by contrast, explanations focus on the roughly one standard deviation of achievement that separates the races, because this is suspected to be the result of discrimination and other environmental disadvantages that are targets of reform. Since variation in measurable school inputs other than facilities is substantial between the races and is statistically related to achievement, schools explain a considerable portion of the $A_w - A_b$ gap. The racial achievement gap is, in turn, only a portion of σ_i^2, which is probably mostly explained by variables that are hard to measure, such as innate intelligence.

A related value-relevant feature of the typical analysis of individual achievement is that all associations between achievement and school characteristics that are correlated with race and class are assumed to be spurious. Coleman (1966, p. 311) says:

> When achievement differs as much as it does between these groups, then to analyze the groups together, without controlling for race or ethnicity of the student, would cause any school characteristics highly associated with race or ethnicity to show a spurious relation to achievement.

In accordance with the assumption that school differences that are correlated with race are not the real causes of achievement differences that are correlated with race, Coleman analyzes variance within each group. In effect, he attributes the disputed variance in achievement to racial background, rather than to schools. A similar assumption justifies the use of "unique contributions to variance" as measures of the "importance" of variables in Table 2 (1972, pp. 160-61), which nets out school effects that are confounded with home background. Thus Coleman's analysis of individual variance fails to ask whether discrimination in the distribution of school resources is responsible for the low achievement of disadvantaged students (Smith, 1972, pp. 239-40). The causal path that represents discrimination in the distribution of school resources ($C \rightarrow S \rightarrow A$) is downgraded by asssumption, which is surprising, given that the study was commissioned to assess "the lack of availability of equal educational opportunities for individuals by reason or race, color, religion or national origin" (*Harvard Educational Review,* 1969, p. 3).

In contrast to studies of individual differences, studies of group differences use the relationships among indicators of race, class and school quality to explain status-linked achievement gaps. Various versions appear in the literature (Jencks, 1979; Duncan, 1968; Bowles, 1973; McPartland and Sprehe, 1973; Mayeske, 1970; Blau and Duncan, 1967). Basically, these studies attribute the relationship between race (or status) and achievement to other variables that are associated with both. Thus the joint variation among achievement, school and race variables that is netted out of explanations of individual achievement becomes the basis for explaining group differences.

Moreover, the version of the between-race method that is responsible for Table 1 (and Table 3 below) follows Coleman's original design (1972, pp. 153-55) and uses regression coefficients for school variables that are estimated with controls for race, class and region. Thus the two tables both employ raw regression coefficients that are calculated with all other variables (controls) in the equation.

Given the common raw regression coefficients, the contrasting findings of Tables 1 and 2 can also be explained in terms of the different weights that are applied to the coefficients. Raw regression coefficients are weighted to provide estimates of the importance of variables in explaining the portion of the distribution of achievement that is addressed by the study. The study of racial differences weights each school variable regression coefficient with the difference between black and white averages for the variable ($\overline{S}_w - \overline{S}_b$). Both the sensitivity of achievement to the school factor and the contrast between majority and minority schools are included in the measure of importance. The study of individual differences weights each school variable regression coefficient with some measure of the variation in exposure to the school characteristic within each race (Werts and Linn, 1969; Coleman, 1972, pp. 159-60). Both the sensitivity of achievement to the school factor and variation in exposure within each race are included in the measure of importance. To paraphrase Coleman, if all whites go to equally good schools, and all blacks go to equally bad schools, then schools will be found to have no importance as explanations of individual achievement differences (1966, p. 310).

As a result of the different weights and the treatment of variance confounded with race, the rank order of variables by policy importance in a study of racial differences tends to follow the rank order of correlations between the school characteristics and race, while just the opposite tends to occur in a study of individual differences. In other words, if the correlation between race and school quality measures is taken to be an indicator of discrimination, then the between-race study is designed to measure the importance of indicators of discrimination, while the within-race study is designed to assess the importance of independent variables, assuming that discrimination does not explain achievement.

Detailed Findings about Black/White Achievement Differences. Table 3, constructed from equations produced by Smith (1972, pp. 331-38) from the Coleman data, allows a more complete exploration of the explanations of racial differences in achievement as of 1966.

Table 3 (bottom) tells us first and foremost that environmental variables can explain black/white differences in scholastic achievement. In the sixth grade, where schools are still relatively homogenous, we find that the average white would lose over half the white advantage over the average black as a result of attending the average black school. If the average white also lost home environment advantages ($S + C$), 81 percent of the remaining achievement advantage would disappear. Clearly, the Coleman data show that environmental factors are able to explain black/white differences in achievement.

Findings also indicate that in 1966 schools alone were important in explaining achievement differences between blacks and whites. In all but the white equation for the twelfth grade, school differences account for substantially more of the achievement gap between the races than does home background. It follows that eliminating school differences remaining between blacks and whites would substantially reduce the black/white achievement gap, even if home environment differences remained. While residual school differences appeared small to Mosteller and Moynihan (1972, p. 11) and others, statistical analysis shows that these differences explained a large portion of the black-white achievement gap.

Alarmingly from a policy perspective, the table also indicates that the achievement of whites would drop roughly twice as much as the achievement of blacks would rise as a result of a complete exchange of school resources in the early school years. While home background variables were far more important for whites than for blacks in 1966, the analysis indicates that they were not strong enough to insulate whites from the effects of significant changes in school quality. These findings, paralleling those of McPartland and Sprehe, suggest that a policy of redistributing existing school resources by moving whites to predominantly black schools and blacks to predominantly white schools (population sizes permitting) might hurt whites more than it would help blacks. This would, however, be an efficient way to achieve the stated goal $\bar{A}_b = \bar{A}_w$.

Despite problems of multicollinearity, which make coefficients unstable when variables in the equation change, it seems useful to use the greater detail of Table 3 to interpret the finding that whites are generally more sensitive to changes in school quality than are blacks. The greater influence of changes in school quality on whites seems to be attributable to variables that refer to race of teacher (18) and race of student (48), which have substantial and contrasting results for blacks and whites. Exposure to majority-black schools has a largely consistent and negative effect on white achievement. But the effects of majority-white schools on black achievement are mixed. If the majority-white student body variable has a positive effect, then the majority-white staff variable

Table 3. Predicted Changes in Standard Units of Achievement for Northern Blacks and Whites That Would Result from Exposure to the Average School and Home Conditions of Members of the Other Race[a]

		Sixth Grade		Ninth Grade		Twelfth Grade	
No.	Variable Name	Black	White	Black	White	Black	White
	FAMILY BACKGROUND FACTOR						
3	Reading Material	+.052	–.077	+.031	–.054	+.016	–.085
4	Items in Home	+.075	–.072	+.025	–.035	+.010	+.000
5	Siblings	+.033	–.058	+.080	–.110	+.058	–.078
6	Structure for Integrity	+.026	–.036	+.037	–.063	+.020	–.021
39	Parents' Education	+.032	–.063	+.028	–.071	+.049	–.066
40	Urbanism	+.000	+.002	–.011	+.019	–.008	+.002
	TOTAL C[b]	+.217	–.304	+.191	–.313	+.144	–.248
	FACILITIES & CURRICULUM FACTOR						
38	Per-Pupil Expenditure	+.001	–.000	–.001	+.007	–.000	+.002
23	Volume/Student	+.004	+.002	–.001	–.003	–.007	+.006
22	Science Lab			+.001	–.002	–.002	–.009
24	Extracurricular Activity			–.002	+.000	+.001	+.001
36	Accelerated Curriculum	+.004	+.000	+.011	+.000	+.007	–.018
25	Comprehensive Curriculum			–.008	+.010	–.001	+.001
35	Tracking			+.005	+.001	+.003	+.000
27	Movement between Tracks			+.002	+.001	–.002	–.001
9	School Size	+.002	+.002	–.001	+.003	+.016	–.011
30	Guidance Counselor			+.004	–.005	–.009	+.031
33	School Location	+.021	–.034	+.006	+.000	–.025	+.033
37	Promotion of Slow Students	+.001	–.001				
	TOTAL S_1	+.033	–.030	+.017	+.013	–.018	+.036
	STUDENT BODY FACTOR						
10	Encyclopedia	+.068	+.015	+.043	–.012	+.045	–.000
26	Student Transfer	–.004	+.016	+.029	–.018	+.027	–.015
31	Attendance	–.039	–.009	+.022	–.059	+.033	–.013
52	College Track			–.032	+.006	+.067	–.033
53	Homework			–.008	+.001	+.027	+.000
55	Teacher Evaluation of Pupils	+.100	–.097				
48	Proportion White	–.021	–.266	+.381	–.250	+.122	–.120
	TOTAL S_2	+.105	–.341	+.435	–.331	+.322	–.181

Continued on next page

Table 3. (continued)

No.	Variable Name	Sixth Grade		Ninth Grade		Twelfth Grade	
		Black	White	Black	White	Black	White
	TEACHER CHARACTERISTICS FACTOR						
11	SES Level	–.000	–.000	+.003	+.000	+.008	+.000
12	Experience	+.003	–.004	+.016	–.004	+.003	+.013
13	Localism	+.079	+.000	+.002	+.003	+.015	+.012
15	Degree	+.009	–.004	–.025	–.007	+.006	–.010
16	Preference	–.007	–.006	+.003	+.027	+.087	–.038
	Student						
17	Verbal	+.006	–.016	+.014	+.007	+.016	–.030
18	Race	+.044	–.165	–.096	–.170	–.078	–.000
	TOTAL S_3	+.134	–.196	–.083	–.145	+.057	–.054
	TOTAL CHANGE	+.489	–.871	+.560	–.776	+.505	–.447
	DUE TO SCHOOL	+.272	–.567	+.369	–.463	+.361	–.198
	DUE TO HOME	+.217	–.303	+.191	–.313	+.144	–.248
	$\bar{A}_w - \bar{A}_b$ in Standardized Units[c]	1.319	1.076	1.159	1.048	1.028	1.060
	$\% \bar{A}_w - \bar{A}_b$ explained by schools, S[d]	21	53	32	44	35	19
	$\% \bar{A} - \bar{A}$ explained by S + C[d]	37	81	48	74	49	42

Source: The table is calculated from equations provided by Educational Opportunity. Marshall S. Smith, "Equality of Educational Opportunity: The Basic Findings Reconsidered." In Frederick Mosteller and Daniel P. Moynihan (eds.), *On Equality of Educational Opportunity* (New York: Vintage Books, 1972). The data are from James S. Coleman, *Equality of Educational Opportunity* (Washington, D.C.: U.S. Government Printing Office, 1966).

[a] The method used in constructing this table is the same as used by James McPartland and J. Timothy Sprehe, "Racial and Regional Inequalities in School Resources Relative to Their Educational Outcomes," *Social Science Research* 2 (1973): 321-32.

[b] Figures do not add precisely to the totals because of rounding. Totals are correct.

[c] $\bar{A}_w - \bar{A}_b$ in standardized units is the difference of average achievement for the two races standardized by the variation in achievement for that race.

[d] The percent of $\bar{A}_w - \bar{A}_b$ explained is the sum of changes in standardized units over the achievement gap in standardized units.

has a negative effect, and vice versa.

These observations from Table 3 are consistent with the results of case studies of integration. In the short run, racial hostility, stereotyping and other holdovers from segregation work against swift improvement in achievement for blacks (National Institute of Education, 1976, pp. 31-47, 90-92; Crain and Mahard, 1979, p. 707). The prediction that white achievement will drop from integration into majority-black schools is also confirmed. This rarely occurs, because of white parent resistance (Crain and Mahard, 1979, pp. 726-27), but when it does, white students suffer (National Institute of Education, 1976, p. 49).

Also, these data support the *Brown* argument that segregated schools are inherently unequal. Both blacks and whites are predicted to do less well in majority-black schools. For blacks, the effects of otherwise effective schools are dampened by continued discrimination, but whites get full benefit of effective majority-white schools, and the predicted negative effects of shifting to majority-black schools are dramatic. This suggests that the performance of the average majority-black school is substantially inferior to the performance of the average majority-white school.

While suggesting that segregated education is "inherently unequal," the findings raise questions about commonly accepted interpretations of the nature of the disadvantage for blacks. The damage from segregated education does not appear to lodge in the attitudes and self-images of black students. If it did, we would not find a dramatic lag in the achievement of white students who are exposed to majority-black schools. The evidence suggests that the average majority-black school is simply less productive than the average majority-white school. This is an institutional problem, not a psychological problem of black individuals.

Impacts. The Coleman Report had enormous influence (Moynihan, 1969, p. 195). First, it convinced many well-informed educators that families were more important than schools in producing achievement. Seymour Lipset is quoted as saying to Patrick Moynihan: "Guess what Coleman's found? . . . Schools make no difference, families make the difference " (Hodgson, p. 35). As a result of the report, policy instruments and explanations for black-white differences in achievement were sought outside the schools, for example in theories of cultural deprivation (National Institute of Education, 1976, pp. 26-27).

Genetic explanations for black/white differences were also advanced. Arthur Jensen (1972, p. 10) notes that he became interested in genetic rather than environmental explanations of racial differences in intelligence because the Coleman Report

> presents massive evidence that discrepancies in educational achievement by different social class and racial groups are correlated to only a slight degree with inequalities in those variables over which schools traditionally have control. The data made it abundantly clear that biological and social environmental factors associated with social class, race and family background accounted for most of the variance in intellectual ability and scholastic performance.

Collins (1978, pp. 172-73) talks about the effects of the report on the movement toward school integration:

> The report stressed two main points. First, the single greatest determinant of a child's academic performance is family background. And, second, if a minority pupil from a home lacking educational strength studies with schoolmates who have strong educational backgrounds, the minority child's achievement will improve—without effecting a negative response in the performance of other children. Thus increasing contact between white and minority students, from a purely educational viewpoint, was not a zero-sum game. . . . The publication of the Coleman Report in 1966 gave fresh impetus to the desegregation drive, which had been stymied by years of southern intransigence. In particular, civil rights groups seized on Coleman's second point in advocating integration through the busing of school children. As a result, the Coleman Report strongly influenced the issuance of the federal busing order of 1970 and was persuasive in subsequent litigation.

But the findings from Coleman's within-race analysis of individual differences in achievement cannot be generalized to explain between-race and between-status differences in achievement. The influential interpretations of the report go beyond its assertions and are largely unsupported. In contrast with the impacts noted above, a between-race analysis of the 1966 Coleman data (Table 3) supports the following general conclusions:

- Environmental factors can and do explain white achievement advantages over blacks;
- School factors explain more of the black/white achievement gap than home background factors;
- In the short run, the achievement of whites who are sent to majority-black schools is expected to drop;
- In the short run, integration will have only a modest, positive effect on black achievement;
- In general, involuntarily segregated black schools are "inherently unequal" because they are less productive than majority-white schools.[5]

Peer Review. The counter-intuitive results of the Coleman Report stirred controversy, and the data were thoroughly reanalyzed (Mosteller and Moynihan, 1972; McPartland and Sprehe, 1973; Averch, 1972; Jencks, 1972; Mayeske, 1970; Harvard Educational Review, 1969). The most influential of these studies were Averch (1972) and Jencks (1972) which, according to Aaron (1978, pp. 72-98), had the effect of reinforcing Coleman's disturbing findings about the ineffectiveness of schools.

Given strong professional norms governing the use of value-relevant assumptions and selections in research, the effect of the reanalyses should have been quite the opposite from that reported by Aaron. Unfortunately, professional standards regarding the use of values in social science are underdeveloped (Strauch, 1976). Thus peer review failed to identify the contradictions between the reformers' policy perspectives and the assumptions required to identify the independent causes of individual differences in achievement. (There were at least two analyses that explained racial differences using the Coleman data, Mayeske, 1970, and McPartland and Sprehe, 1973, but these were not influential.)

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[5] It is worth noting that the "education production function" literature rarely deals with contributions to achievement that result from improved use of fixed resources—for example, from better management of highly qualified teachers so that they, in turn, have more effect on student achievement. In effect, the productivity of the average school is tacitly assumed to be good enough, and policy recommendations to reduce the black/white achievement gap focus on input levels. Some analysts have questioned this approach and have tried alternatives (Klitgaard and Hall, 1973; Levin, 1976). Our normative dynamic model also calls for improved school productivity, because we argue that closing the black/white achievement gap requires that good schools be at least effective in teaching blacks as they are in teaching whites. We should note in this context that the expectation for improved productivity could be applied to all-black schools as well as to integrated schools. In a society devoid of discrimination, attendance at an all-black school would not be a disadvantage.

In one of the influential analyses, Averch (1972) promises to assess the internal and external validity of studies of school effects, including Coleman's, by asking: "Did the researcher use methods appropriate to the problem he addressed?" (p. vii), and "Were results credible in the light of accumulated knowledge?" (p. v). Given the existence of a well-established methodology for studying between-race differences (Blau and Duncan, 1967; Duncan, 1968), the inclusion of between-race studies among the reanalyses (Mayeske, 1970; McPartland and Sprehe, 1973) and Coleman's original plan to execute a between-race study (Coleman, 1972, pp. 153-55), Averch seemed likely to identify problems, but did not do so. Instead, his conclusions about "input/output" studies, which included Coleman's, reiterated findings that schools have little effect, and are less important than social background (Averch, 1972, pp. 44-45). The other influential review (Jencks, 1972, p. 14) advocated the policy goal of equalizing individual differences in achievement and executed an appropriate analysis. Jencks did not, of course, contradict Coleman's major findings, since his statistical approach was roughly the same as Coleman's.[6]

Thus, peer review did not convincingly reveal and correct the failure to match the client's policy question with the research approach. Even today, research-informed individuals generally believe that family and other non-school advantages, independent of schooling, explain the superior educational achievement of whites and upper-class children over the "socially disadvantaged." The entire "Coleman episode" is an embarrassment to social scientists. To avoid similar embarrassments, especially in dealing with questions of equity, social scientists need to develop professional norms for assessing the match between statistical articulations and policy issues.

Toward a Normative Dynamic Model of Integration for 1966

Because studies of racial differences explain the black/white achievement gap, they are useful in identifying potential policy variables to reduce the gap. Analyses of individual variation in achievement are not helpful in identifying the causes of the achievement gap, because they produce a rank-order of variables by importance that is the reverse of what it should be. But, ironically, between-race studies produce poor predictions about the long-term effects of integration, while the apparently non-responsive within-race approach makes reasonable long-term predictions. Actual case studies of integration show that white achievement is largely unaffected by integration, while black achievement is raised modestly over time (Crain and Mahard, 1979, p. 707). This outcome is consistent with the results of studies of individuals (Table 2), but it is out of line with the predictions, from studies of racial differences, that white achievement will drop faster than black achievement will rise (Tables 2 and 3).

Poor predictions suggest that the simple normative model we have used to explain racial differences in achievement must be modified. First, we must add explicit constraints that reflect political pressures and resource limitations. A usual constraint in school equalization is to hold advantaged students "harmless"—that is, to assure no deterioration in their schooling. It is the operation of this constraint that invalidates the predictions from the simple between-race model. White achievement would decline if whites were exposed to school conditions faced by the average black, but political pressures do not allow this to happen (Crain and Mahard, 1979, pp. 726-27). The simple model identifies the probable causes of black/white differences in achievement, but it is a poor policy simulator, because it does not deal with constraints that occur in implementation (see Hargrove, 1975). This point is discussed in more detail below.

To fully represent the reform objective, the model must also contain modified raw regression coefficients that relate achievement to merit, institutional variables, and background characteristics. One goal of integration is to foster equal opportunity—that is, to alter the rules of the game that govern attainment. These processes or rules are reflected in regression coefficients (Duncan, 1968). For example, reflections of discrimination and segregation are seen in the findings of Table 3, which indicate that regression coefficients are lower or mixed for blacks. Specifically, $p_{as.c} < q_{as.c}$ (school multipliers are smaller for blacks), and $p_{ac.s} < q_{ac.s}$ (home background multipliers are smaller for blacks). Also, other research implies that $p_{am} < q_{am}$) multipliers of individual merit are smaller for blacks (National Institute of Education, 1976, pp. 56-57, 86-94, 151-56; Heyns, 1976; Jensen, 1968; Miller, 1981).

If regression coefficients reflect segregation and are targets of reform, then an analysis that treats them as empirical laws makes the conservative assumption that caste and class biases cannot be altered. Thus a normative, dynamic model of integration must include the expectation that regression coefficients as well as school inputs will be equalized (Miller, 1981).

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[6] This statement applies to Jencks, 1972, not Jencks, 1979. *Who gets ahead?* focuses primarily on differences in attainment between groups from different social backgrounds. Results of Jencks' between-race status analyses conform to prior studies using the approach. Jencks does not make sloppy connections between values and research methods.

In the brief sub-sections that follow, we explicitly modify the simple between-race educational reform model to reflect resource constraints and expected changes in the rules governing "who gets what" in the attainment process. We also illustrate possible applications of the normative model.

Constraints on Resource Equalization. The symbolic language of reform implies pure equity objectives—that schools, and ultimately achievement, should be equal for blacks and whites ($\bar{S}_b = \bar{S}_w$ and $\bar{A}_b = \bar{A}_w$). No constraints are stated. But in the specifics of legislation and in implementation, equity objectives are often swamped by constraints—concessions to affluent interests and resource limitations. The result can be "symbolic reform" (Edelman, 1971, 1977). An effective constraint in school reform has been $\bar{S}_{w,t+n} \geqslant \bar{S}_{w,t}$—that is, whites must not lose school resources in the reform, even if this slows equalization. Operation of this constraint causes a departure from efficient realization of Min ($\bar{A}_w - \bar{A}_b$). Looking back at Tables 1 and 3, we see that the stated, unconstrained equity goal could be accomplished most swiftly by sending whites to majority-black schools, within the limits of numbers. This would lower white achievement dramatically, raise black achievement slightly, and require minimum commitment of new resources to education.

As a practical matter, reforms to Min ($\bar{A}_w - \bar{A}_b$) are not achieved through redistribution of existing school resources. Rather, in both school finance reform and school integration, attempts are made to maintain high-quality education for advantaged children, and new resources are dedicated to disadvantaged children (see Orfield, 1978). Indeed, studies of the politics of educational reform tell us that it is generally necessary to put together a benefit package so that representatives of advantaged students have some incentive to go along with the "reform" (Garms, Guthrie, and Pierce, 1978, pp. 356-57; Odden, 1978, p. 146). But achieving equalization by committing new resources is inefficient—it takes more time and money than are required to meet the objective. Indeed, if political realities indicate that advantaged students must gain as a result of "reforms" to help the needy, the operational political constraint may be $\bar{S}_{b,t+n}/\bar{S}_{w,t+n} = \bar{S}_{b,t}/\bar{S}_{w,t}$—no equalization of school resources. Moreover, operation of this constraint implies substantial growth of the public sector if the "reform" is pursued.

Limits involving commitments to professional interest groups, as well as other factors, further constrain achievement of equity goals. Because many of these resource constraints can be anticipated at the time a reform is mandated, policy analysts should lay out an expected timetable for progress toward school equalization and reductions in the black/white achievement gap. If constraints reduce expected school equalization to insignificance, it is misleading to justify expenditure increases in terms of "equal opportunity" to help blacks achieve parity with whites.

Rules Governing "Who Gets What." Historically, discrimination kept most blacks in second-class occupations, regardless of individual merit, talent, and contribution to the economy. Thus, whites were differentiated according to their potential for society-wide leadership, while blacks were prepared for leadership within the oppressed black caste. One result was that white educational achievement ran from very low to very high, while most blacks were bunched together at low-to-moderate levels (Jensen, 1972, p. 161).

If we assume an equal distribution of genetic ability between the races, then ending caste discrimination requires equal differentiation among blacks and whites according to their talent (Thurow, 1972). High-ability blacks must achieve leadership at the same rates as equally able whites. These expectations about changes in the rules of the game that accompany desegregation must be incorporated in our statement of the reform objectives of school integration. In addition to Min ($\bar{S}_w - \bar{S}_b$) (redistribute resources) we must Min ($q - p$) (reduce institutional barriers to upward mobility). If raw coefficients continue to be lower or more mixed for blacks as compared to whites, achievement equalization ($\bar{A}_b = \bar{A}_w$) cannot be attained despite input equalization ($\bar{S}_b = \bar{S}_w$ and $\bar{C}_b = \bar{C}_w$).

More specifically, in designing a normative model of integration for the 1960s, we would insist that white expectations for black achievement go up, so that exposure to good majority-white schools has strong favorable effects on blacks (National Institute of Education, 1976, pp. 79-110). This objective implies $p_{as} \geqslant q_{as}$ for indicators of school quality—that is, black achievement should be at least as sensitive to good schools as white achievement. Also, successful black parents should have at least the same ability to boost the educational achievement of their children as successful white parents. Thus, regression coefficients with family background would have to go up so that $p_{ac} \geqslant q_{ac}$. (Jencks, 1979, pp.

192-212, indicates that this is one area which has shown substantial improvement since the 1960s.) Finally, and most importantly, individual blacks who have the same innate ability as whites must be encouraged to achieve at levels that are at least equal. Thus regression coefficients that relate measures of learning ability to achievement have to go up. The objective is $p_{am} \geqslant q_{am}$. (Learning ability is not measured in the Coleman data.)

Applying the Model. A normative, dynamic model for integration should be useful in policy analysis, program evaluation, implementation and basic research on political change. Some examples of uses that might be made of such a model are included below.

The research applications of a normative model of integration include explaining the causes of black/white differences in achievement. Results in Table 3 suggest that the findings as of 1966 point primarily to school and attainment process disadvantages. While results are not definitive in terms of causal effects, they identify the probable explanations of the black/white achievement gap.

Second, policy analysis (taking implementation into account) could be used to specify expectations for reform, year by year, given constraints. Expectations would involve changes in both school factor levels and regression coefficients. Political constraints, such as protecting white students' advantages, should be taken into account explicitly, along with total resource limits.[7] This would provide a realistic schedule for input equalization. Political pressures that constrain expectations for changes "in the rules of the game," or raw regression coefficients, should also be specified. For example, if schools intend to protect affluent whites of modest talent from displacement by more talented blacks in college-bound classes, projected rates of upward mobility for blacks will be depressed, unless total college attendance increases dramatically.

Timetables for input and mobility equalization produced by policy analysis could be used in program evaluation, and in studies of implementation and accountability. For example, progress predicted by a constrained reform model could be compared to actual progress to monitor implementation. As predicted by a realistically constrained model, progress toward school equalization would be very slow.

Alternatively, critics of the status quo could model the reform without constraints to point out the degree to which policies to equalize achievement for blacks are "symbolic." This would be easy to do in the case of integration. For example, we noted that efficient achievement of $\overline{A}_b + \overline{A}_w$ would result from redistribution of existing school resources—say, two-way busing—which implies $\overline{A}_{w,t+1} < \overline{A}_{w,t}$ (a drop in achievement for whites) in the short run. The expected cost and progress toward $\overline{A}_b = \overline{A}_w$ under this efficient reform could be compared to the observed cost and progress of the actual, politically constrained, reform. Public sector costs above the efficient level would be attributed to maintaining the status quo or conservatism, not to equalization. Also, persistence of $\overline{A}_w - \overline{A}_b$ at pre-reform levels could be attributed to the maintenance of white advantages in schooling.

In addition to monitoring and assessing implementation, normative, dynamic models might be used to encourage reform by suggesting performance targets. Some schools can be expected to be "ahead of their time" in achieving integration. Parameters for indicators of merit-recognition could be calculated within these schools. Results should show more efficient use of inputs and black student assets, and would be used to target $p_{t+1} > p_t$ (a rise in regression coefficients in black equations) for less progressive schools. This technique could be used to encourage greater efficiency in the use of resources in majority-black as well as integrated schools, because 1966 and later data show that institutions that serve the disadvantaged are not as productive in their use of resources (parents, teachers, student skills) as are institutions that serve the advantaged.

Finally, normative dynamic models could be employed in basic research on political change. We have hinted at their use in identifying "symbolic reforms" and in explorations of the causes of public sector growth. In addition, the failure of policy makers (and policy analyses) to seriously consider achieving equity through redistribution of existing resources from whites to blacks may be an example of a "non-decision."

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[7] In a sense, the Coleman Report did take constraints into account, but it did so implicitly. The implicit constraint was that there would be no radical change in the distribution of school resources. After reform, schools would continue to be uniform and fairly good for whites, but would vary more for blacks, due to black attendance at majority-white schools. In other words, analysis of variance in individual achievement within each race "simulates" a "reform" under conditions of no significant redistribution of school resources that would hurt whites. While this outcome isn't what reformers say they have in mind, it captures their behavior. The damage from Coleman's use of implicit, rather than explicit, constraints in policy analysis was the apparent (false) finding that the major causes of the black/white achievement gap were genetic and social, not institutional (see Jensen, 1972, p. 10).

Implications for Political Science

Our review of the literature on educational equity lends support to two observations. First, political scientists are not fulfilling their unique responsibility among social scientists to develop empirical theories, concepts and measures that incorporate political objectives and realities into policy analyses. Second, the failure stems from the fact that political ideas, such as equity and accountability, cover more behavioral possibilities than can be observed in a given locality or point in history, yet empirically oriented social scientists try to limit research to the study of observed events (Merelman, 1968; Wolfinger, 1971).

More specifically, review of the interdisciplinary literature on educational equity indicates that social scientists have difficulty interpreting empirical methods and findings within appropriate political contexts. Political scientists fail to do this in their own work (Wahlke, 1979), which leaves a gap for all of social science. Thus, as we saw above, Coleman's otherwise sophisticated study of equal educational opportunity failed to deal explicitly, rigorously and appropriately with political goals and constraints.

While supporting Wahlke's excellent criticisms (1979), we advocate a direction for political science that differs from his recommendation to study pre-programmed, genetically determined individual behavior. Along with building up political theory from biology and basic human drives, we recommend a top-down approach that measures concepts like "symbolic politics," "accountability" and "non-decisions" by comparing expectations for reform with observed events.

We advocate this partially normative approach because we believe that human beings can imagine new social relations that are not yet true and may never be true, and that these visions, executed through political institutions, are the driving force of history. We also believe that advantaged groups turn to political institutions to preserve and enhance their status. Thus, to be understood, current behavioral outcomes must be viewed in the context of reformers' visions, legal entitlements, and conservative forces. Equally important, the long-run future must be predicted from knowledge of what is wanted, as well as of what is.

From this perspective, we recommend that normative, dynamic, models be used to quantify moral visions, foreign examples, and empirically referenced political theories. Such models would have use in basic as well as applied research. In applied research they would forecast desired changes in the distribution of resources with and without constraints expected in implementation. One use would be to clarify goals and encourage goal achievement. In basic research, comparisons of the normative expectations of various interests to actual events could be used to identify political winners, independent of symbolic statements about political outcomes. Where political constraints negate the stated policy, lack of political accountability could be identified. Growth of the public sector resulting from actions that maintain established advantages and undercut progress toward equity could be properly attributed to conservatism, rather than to reform. Finally, empirical research, both basic and applied, could be freed from the conservative bias that results from exclusive reliance on observed relationships.

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The Allocation of Federal Aid Monies: The Synthesis of Demand-Side and Supply-Side Explanations

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Previous research on the distribution of federal aid monies has been dominated by the donor's perspective. Different distribution formulas, political influence of congressional representatives, bureaucrats, and individual aid recipients have been studied as the sole determinants of aid allocations. Each explanation, however, fails to examine the question of aid allocations from a demand-side perspective. This omission assumes that all governmental units are equally desirous of federal assistance and that any bias in the distribution of federal aid is a function of supply-side conditions. Identifying a linkage between demand-side and supply-side determinants of aid allocations, this article proposes and tests hypotheses derived from an integrated model of federal aid allocations.

The current debate over the allocation of federal aid to state and municipal governments has produced a number of attempts at explaining who gets what, where, when, and how (Havenman et al., 1976). The dominant explanatory approach has been to adopt the perspective of the donor—the federal government. Here, different distribution formulas, political influence of congressional representatives, bureaucrats, and individual recipients have been studied as determinants of aid allocations. Each explanation, however, fails to examine the question of aid allocation from a demand-side perspective. This omission assumes that all governmental units are equally desirous of federal assistance and that any bias in the distribution of federal aid is a function of supply-side conditions.

Identifying a linkage between demand-side and supply-side determinants of aid allocations, this article proposes and tests hypotheses derived from an integrated model of federal aid allocations. The following questions serve to further focus this study:

1. What has been the historical pattern in the allocation of federal aid? Has federal aid been allocated equally to all recipients or has there been a specific bias in the pattern by which federal monies are distributed?
2. How has the grant application process (i.e., demand behavior) affected the distribution of federal aid to municipal governments?
3. To what extent does the degree and nature of prior experience with the federal aid system structure grant seeking and receiving?

Supply-Side Explanations of Aid Allocations

Supply-side explanations of federal aid allocations have emphasized the donor's perspective (i.e., that of the federal government), often at the neglect of the recipient's viewpoint. Supply-side theorists, however, have not been uniform in identifying determinants of federal aid outlays. Congressional scholars (Anagnoson, 1979; Ritt, 1976; Strom, 1975; Murphy, 1974; Ferejohn, 1973, 1972) have long argued that representatives with key committee appointments, seniority, and other positions of relative power, use their influence to obtain a disproportionate amount of the federal largesse for their congressional districts. In spite of the intuitive appeal of this hypothesis, supporting evidence has been lacking. The chronology of research findings suggests a waning of traditional political influences on the allocation of federal aid monies.

A recent trend in supply-side explanations of aid outlays has been the substitution of bureaucratic for congressional authority over the distribution of federal largesse (Anagnoson, 1978; Gilbert and Specht, 1974). A majority of aid programs delegate responsibility for awarding assistance to agency administrators (Anagnoson, 1978, p. 34). Research on bureaucratic influence suggests that administrators use their authority in a timid and non-systematic fashion. Studying

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EDA's public works program, Anagnoson observes (1978, p. 34):

> The criteria the agencies use to select projects reinforces the restraints through their vagueness and ambiguity . . . the standards the regional directors use are so loosely specified that they do not determine the final distribution of projects.

Even in instances where administrators engage in overt and specific targeting of aid, there is little evidence to support the success of their efforts. Gilbert and Specht (1974) found that Model Cities grants allocated on the basis of the "applicants most likely to achieve Model Cities Program objectives," failed to achieve the desired outcome. The results indicated that cities awarded Model Cities monies were no more likely to achieve the desired level of performance than non-recipients.

A Demand-Side Explanation of Aid Allocations

Each of the supply-side explanations assumes that (1) all municipal governments are capable of applying for federal aid monies and (2) that those municipalities outside the federal aid system have no specific need or, for reasons of political preference, choose not to apply for federal aid. A number of researchers have suggested that not all potential recipients are equally capable of seeking and receiving federal assistance. Porter, Warner, and Porter (1973), Saltzstein (1977), Gabris and Giles (1978), Reed and Green (1978), and Stein (1979) have empirically demonstrated that there are significant obstacles to grant seeking which indirectly structure allocation patterns. Certain non-transferable costs (i.e., application, implementation, and opportunity costs) confront the potential applicant for federal assistance. These costs adversely affect grant seeking activities of smaller, fiscally and socially needier communities, effectively eliminating them from the pool of eligible aid recipients and reducing the potential for equalization in the allocation of federal monies.

Many needy and desirous municipalities find the costs of seeking and receiving grant assistance greater than the potential benefits of participation in the federal aid system. The costs associated with grantsmanship, however, do not operate independently of supply-side factors to structure grant allocations. Demand theorists have essentially described a process in which potential applicants respond to specific conditions of the grant system. Matching requirements, administrative oversight, and requisite program activities provide the basis upon which potential applicants decide whether or not to seek and/or receive federal aid.

An Integrated Explanation of Grant Allocations

Proponents of a demand-side explanation of grant allocations have provided an important insight into the operation of municipal grant-seeking activities. However, there remains a significant gap in their research. How does the application process operate to structure grant seeking over time? Specifically, what is the nature of the process operating to encourage or discourage communities from seeking grant assistance at one time, while producing the opposite effect on grant-seeking activities at some later time?

The administrative structure of federal aid programs gives the prospective applicant information with which to assess the merits of seeking and receiving federal assistance. Certain aid packages stimulate grant seeking, while other aid programs have either a neutral or negative effect on grantsmanship. Project grants require formal applications and are competitively awarded by administrative officials. In varying degrees, matching expenditures and compliant policy behavior are required of all aid recipients. Consequently, some communities find project grants difficult to apply for and politically undesirable to accept. Revenue sharing and block grant aid mechanisms have gradually replaced many project grants with a set of functionally integrated formula-based aid programs. Receipt of formula-based assistance is not subject to competitive grant applications, significant matching funds or excessive policy requirements. The result is a more desirable aid mechanism that is both substantively and administratively attractive to municipal governments.

The aid programs of President Johnson's Great Society (1963-1968) were dominated by restrictive and costly project grants (ACIR, 1978, p. 40). Great Society project grants may have discouraged many municipalities from seeking federal assistance, since the costs associated with applying for such assistance and the requirement for fulfilling grant obligations outweighed any probable benefits associated with receipt of federal aid. In contrast, the New Federalism aid packages initiated by the Nixon administration (1969-1977) were dominated by more flexible formula-based block grant and revenue-sharing aid programs. These less restrictive and less expensive New Federalism programs made it possible for many nonrecipients to enter the federal aid system for the first time. Moreover, their positive experience with general revenue sharing and block grant programs led many municipalities to experiment with project grants. Indeed, as Loucks has suggested (1978, p. 7):

> Without the New Federalism, many small communities would still be outside the intergovernmental system. With GRS and block grant funds used as bait, cities . . . have become "hooked" on federal grants and, with each passing year, become unlikely to give them up.

The positive experience of municipal governments with New Federalism programs and their newly acquired dependence on federal assistance led these new aid participants to apply for project grant assistance. The nature of this dependence is both political and economic in character. Unrestricted expenditures of block grant and revenue-sharing monies allowed many communities to substitute federal monies for municipal revenues, effectively reducing taxes (Caputo and Cole, 1977; Urban Institute, 1979). In order to maintain stable tax rates, municipal governments were forced to broaden their involvement with the federal aid system and compete for project grant monies. In addition to the political advantages of reduced taxes, many New Federalism programs stimulated demand for additional government services. Citizens provided new services under the Community Development Block Grant program had their appetites whetted for continued and expanded services, requiring new municipal revenues. Unable to raise taxes, the affected governments began to seek federal aid beyond the scope of revenue sharing and block grants (ACIR, 1980b, p. 174).

Three hypotheses can be derived from the previous explanation of federal aid allocations:

1. The degree of equalization in the allocation of federal aid is positively related to the level of municipal participation in the federal aid system.
2. Entry into the federal aid system after 1967 (i.e., after the implementation of revenue sharing and block grants) is inversely related to the participant's socioeconomic status.
3. Participation in the federal aid system is progressive, initially limited to formula-based aid programs with successive expansion into project grant programs.

The first hypothesis conflicts with a number of studies which have attempted to show that general revenue sharing and block grants have served to decrease equalization in the allocation of federal assistance. Dommel (1975) maintains that broadened eligibility under the block grant and revenue-sharing programs provided aid to municipalities which previously were outside the federal aid system. The impact of this newly enlarged pool of eligible recipients is that "the total central city formula entitlement [for the Community Development Block Grant] of $1.2 billion a year is nearly 20 percent lesss than the $1.48 billion these cities received under the categorical (i.e., project) programs" (Dommel, 1975, p. 170).

Dommel's analysis presumes that the new participants in the Community Development Block Grant program and other New Federalism aid programs need federal assistance less than the previous recipients of federal project grants. Our second hypothesis suggests the opposite. Increased participation in the federal aid system is biased toward socially and fiscally needy municipalities, effectively increasing the degree of equalization in the allocation of federal assistance.

Data and Research Design

The population of municipal governments studied is limited to all general-purpose governments over 25,000 in population as of 1967 (N = 810).[1] The latter qualification is necessary to ensure a comparable sample across each year studied (i.e., 1967, 1972, and 1977). Communities under 25,000 must be excluded because many aid programs limit eligibility to communities 25,000 and larger, and the data on socioeconomic characteristics for smaller cities is not collected in a systematic or timely fashion by the Bureau of the Census.

Data on the allocation of federal aid monies are available from the census bureau's *Census of Governments.* The census bureau provides aid allocations by seven functional categories for all general-purpose governments. The census figures, issued every fifth year ending in "2" and "7," include only direct federal aid to municipal governments and exclude monies passed through the state to the municipality. Data on each independent measure of social and fiscal need were available for the three years studied. In the case of several population-based measures (i.e., for the aged and for school-age children), the data are estimates from the 1970 census (U.S. Bureau of the Census, 1979). A fuller description of data sources for federal aid outlays can be found in the appendix; Anton (1979, 1978); Anton, Cawley, and Kramer (1980); and in Hines and Reid (1977).

The appropriate indicators of social and fiscal need are widely debated by both academicians and politicians. Recent legislative battles over the measure of housing stock for inclusion in the distribution formula for the Community Development Block Grant underscore the political importance associated with this issue (Stanfield, 1979). It is not our intent to add to this already growing debate. Selection of need and fiscal capacity measures are based on their relevance to the operation of the federal aid programs studied. The indicators selected are in fact employed in a wide number of federal project and formula-based aid programs.[2] Our measures of a municipality's need and fiscal capacity are as follows:

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[1] Thirty-five cities in California below 30,000 in size were excluded from the analysis because of missing federal aid data.

Factors Measuring a Municipality's Need:

- Aged (percent population over 65 years of age)
- Dependent children (percent population below 18 years of age)
- Education (percent population with high school education, 25 years of age and over)
- Unemployment (percent adult population unemployed)
- Poverty (percent of families below poverty income level)
- Crime (crimes per 1,000 persons)
- Income (median family income)
- Population size

Factors Measuring a Municipality's Fiscal Capacity and Effort:

- Home ownership (percent of dwellings owner occupied)
- Home value (median value of owner-occupied home)
- Property tax rate (full value municipal property tax)
- Tax burden (percent of per capita income paid for taxes[3]

Findings

The first hypothesis implies that changes in the distribution of federal aid are partially a function of a significant increase in the number of municipal governments participating in the federal aid system. Table 1 presents the percentage of communities participating in the federal aid system between 1967 and 1977 by functional area. In each successive year the percentage of participating governments increases. Between 1967 and 1977, 46.7 percent of the communities studied initially entered the federal aid system. Moreover, this dramatic increase is not merely a function of the passive general revenue-sharing program. Of those communities studied, more than 90 percent participated in project grants in 1977, suggesting that at least since 1972 (the first year of general revenue sharing), a large number of municipal governments have broadened their use of aid programs to include project grants.

Participation in each functional category of federal aid increased during the period studied. Interestingly, overall participation in each of these functional areas lags behind participation in all aid programs and does not exceed 50 percent of the communities studied in any year. Limited use of federal aid in at least five areas is due to the absence of municipal functional responsibility for these functional activities. Education, public welfare, employment, and highways are functional responsibilities associated with state and county governments and special districts instead of general-purpose municipal governments. This condition would suggest that participation in the federal aid system may be related to the scope of municipal functional responsibility.

Table 3 presents the relationship between per capita federal aid outlays and indicators of need and fiscal capacity.[4] For comparative purposes,

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[2] See ACIR (1978). There is a tendency when evaluating the targeting capacity of federal aid allocations to measure equalization against more objective and sensitive measures of need and fiscal health than those employed by federal officials. The exploration for more isomorphic measures of social and fiscal need raises a different question than the one addressed in this article. To evaluate targeting in terms of criteria other than those employed by federal authorities is to engage in a biased analysis of federal targeting efforts. In order to evaluate properly the effectiveness and correlates of federal targeting activities, it is necessary to consider the indicators actually employed in distributing the federal largesse, rather than an alternative set of indicators, regardless of their validity.

[3] Tax burden is calculated by dividing the per capita tax revenue from 11 sources by per capita personal income.

[4] There is a growing debate over the appropriateness of per capita aid measures when examining the allocation of federal grant assistance. Uslaner (1976) and Ward (1980) both argue that there must be a theoretically compelling reason for the use of standardized aid measures. Ward notes that aid monies are not allocated by states or the federal government on a per capita basis, and therefore evaluations of these aid programs employing per capita or percentage measures produce distorted results. Though we agree with the logic of this argument, our own analysis fails to produce a significant empirical difference between per capita and total aid measures. Both dependent measures of aid allocations were employed in separate analyses and failed to produce different results. Though the coefficients are obviously different due to differences in measurement, the pattern of equalization observed for both dependent measures is virtually identical. This should not be taken

Table 1. Percent of Municipal Governments Participating in the Federal Aid System by Program and Year: 1967, 1972, and 1977

Program	1967	1972	1977
All Aid Programs	53.3	63.3	100.0 (94.1)*
Education	11.7	11.7	12.3
Employment	.1	.2	.2
Housing/Urban Renewal	25.0	29.9	38.7
Highways	2.5	1.9	6.9
Health	8.2	7.3	10.0
Public Welfare	.4	1.8	2.1
Other	38.8	53.3	90.1
N = 810			

Source: Compiled from U.S. Bureau of the Census, *Census of the Governments: Finances of Municipalities and Township Governments*–1967, 1972, and 1977 (Washington, D.C.: U.S. Government Printing Office).

*Excluding participation in general revenue sharing.

the unstandardized and standardized coefficients and *y*-intercept are presented. Interpretation, however, is limited to the unstandardized coefficient.[5] Nine of eleven need and fiscal capacity measures produced the expected change in the observed relationship with per capita federal aid outlays. Substantively, this indicates that in each successive year a unit change in need and/or fiscal capacity produced a greater change in federal outlays in the hypothesized direction. Fiscal capacity variables appear to lag behind need indicators in the magnitude of their impact on changes in aid outlays. Unit changes in median income and home value each produced less than a $1.00 change in per capita dollar outlays in any year. The coefficients for median home value in each year were insignificant, indicating that home value makes no contribution to the level of equalization in the allocation of federal assistance.

Population size has a significant but extremely weak influence on the allocation of federal aid in 1967 and 1972. As expected, larger cities received more aid, though this ratio is negligible. In 1977, however, this relationship is reversed. There is a moderate and negative relationship between city size and aid allocations. This change reflects the broadening of eligibility requirements to include cities under 50,000 in size in aid programs (e.g., discretionary funding under the Community Development Block Grant). Beyond the political

Table 2. Mean Per Capita Direct Federal Aid Allocation by Year: 1967, 1972, and 1977

	Year		
	1967	1972	1977
Allocation	$4.47	$9.61	$56.02 ($39.95)*
N =	431	513	810

Source: Compiled from U.S. Bureau of the Census, *Census of the Governments: Finances of Municipalities and Township Governments*, 1967, 1972, and 1977 (Washington, D.C.: U.S. Government Printing Office).

*Excluding general revenue sharing allocation.

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as an endorsement of the use of per capita measures. We have reported per capita measures because of their ease of interpretation, not because they are more theoretically meaningful.

[5] I have deviated from previous analysis procedures (Dye and Hurley, 1978; National Governors' Association, 1980) by relying on the unstandardized regression coefficient rather than the (standardized) correlation coefficient to measure equalization over time. Comparisons between standardized coefficients cannot be made over time, since the means and variances, the hypothesized source of variation in the observed correlations, are *invariant* (i.e., mean = 0, variance = 1). The unstandardized coefficient, which does not alter the mean or variance of either variable, allows us to compare the magnitude of change in per capita aid outlays as a function of a unit change in the same variable measured over time. Substantively, the unstandardized regression coefficient is interpreted as the dollar increase in per capita allocations associated with a unit change in need or fiscal capacity. For a further discussion of this issue, see Blalock (1971).

Table 3. Relationship between Per Capita Direct Federal Aid and Selected Indicators of Socioeconomic Need by Year

Variable	1967			1972			1977		
	B	Beta	Intercept	B	Beta	Intercept	B	Beta	Intercept
Aged	21.80*	.087	2.26	48.80*	.092	4.65	269.05*	.186	28.69
Dependent Children	24.65*	.114	12.00	52.93*	.115	25.75	208.11*	.167	83.04
Education	–9.90*	–.107	10.23	–26.14*	–.134	24.27	–130.01*	–.245	128.96
Unemployment	39.91	.064	2.85	105.87*	.080	5.30	769.41*	.215	24.73
Poverty	21.09*	.091	2.72	63.39*	.129	4.34	339.10*	.255	27.81
Crime	140.03*	.217	.67	362.11*	.264	.21	1,254.73*	.237	21.97
Home Ownership	–21.43*	–.255	17.56	–49.76*	–.278	40.00	–170.90*	–.352	160.40
Home Value	.00	.016	3.96	.00	–.026	11.41	.00	.079	85.59
Median Family Income	.00	–.074	8.18	–.00	–.069	20.14	–.07*	–.256	129.40
Tax Burden	738.11*	.343	–.54	514.55*	.366	–.55	815.10*	.523	.61
Population Size	.01	.015	3.86	.05*	.111	3.85	–1.09*	–.339	.00
N=810									

Source: Compiled from U.S. Bureau of the Census, *Census of the Governments: Finances of Municipalities and Township Governments 1967, 1972, 1977* (Washington, D.C.: U.S. Government Printing Office), and U.S. Bureau of the Census, *County and City Data Book: Consolidated File City Data 1944-1977 (Washington, D.C.: U.S. Government Printing Office), 1978.*

*The coefficient is at least twice the size of the standard error and significant at the .05 level.

causes of this change,[6] there is an implicit assumption that "smaller cities" (25,000-50,000) are capable of suffering from the social and economic ills of urban decay. Moreover, this change represents a departure from defining need solely in terms of size, and a sincere attempt to identify the causes of social and economic decline. The findings in Table 3 are not sufficient to demonstrate that changes in equalization are due to increased participation in the federal aid system by needier and fiscally strained municipalities. The covariation of these two variables and their independent effect on equalization must first be established before we can conclude that increased grant use increases equalization.

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[6] See Beer (1976) for a further discussion of this issue.

Table 4. Difference of Means Test between New and Old Federal Aid Participants for the Periods 1967-1972 and 1972-1977

Variable	1967-1972		1972-1977	
	New Participants	Old Participants	New Participants	Old Participants
Aged (%)	10.6	9.6*	10.2	8.9*
Dependent Children (%)	35.3	32.6*	34.0	33.2*
Education (%)	51.2	58.2*	55.8	59.2*
Unemployment (%)	4.4	3.6*	4.1	2.8*
Poverty (%)	9.1	7.3*	11.9	9.1*
Home Ownership (%)	57.5	65.4*	60.7	66.1*
Home Value ($)	17,191	19,033*	17,850	20,347*
Median Income ($)	9,648	10,692*	10,034	11,257*
Tax Burden (%)	6.5	4.2*	7.1	5.2*
Crime (per 1,000)	3.0	2.1*	6.0	5.2*
Population	46,634	160,339*	37,883	113,752*
N =	153	360	297	513

Source: Compiled from U.S. Bureau of the Census, *Census of the Governments: Finances of Municipalities and Township Governments* 1967, 1972, and 1977 (Washington, D.C.: U.S. Government Printing Office) and U.S. Bureau of the Census, *County and City Data Book: Consolidated File City Data 1944-1977* (Washington, D.C.: U.S. Government Printing Office), 1978.

*P ⩽ .05.

Table 5. Patterns of Participation in the Federal Aid System: 1967-1977

Pattern	1967	1972	1977	Percent	N
Constant Participants	Participant	Participant	Participant	44.4	360
New Participants in 1972	Non-Participant	Participant	Participant	18.8	153
New Participants in 1977	Non-Participant	Non-Participant	Participant	27.7	225
Deviants	Participant	Non-Participant	Participant	8.9	72

Source: Compiled from U.S. Bureau of the Census, *Census of the Governments: Finances of Municipalities and Township Governments,* 1967, 1972, and 1977 (Washington, D.C.: U.S. Government Printing Office).

Table 4 presents the mean values on indicators of social and fiscal need for new and old aid participants. The second hypothesis suggests that those entering the federal aid system for the first time (i.e., in 1972 and 1977) were significantly needier than those communities already in the system. New aid participants in both 1972 and 1977 are significantly needier on all measures. The evidence suggests that greater equalization in allocation of federal aid came about as a result of changes in the composition of recipient governments. New recipients added a significant degree of heterogeneity and need to the population of federal aid recipients, thus enhancing the degree of equalization.

Our third hypothesis predicted that new participants in the federal aid system have a progressively wider involvement in the federal largesse. Initially, utilization of aid programs is limited both in quantity (i.e., total dollars received) and scope (i.e., block grants and general revenue sharing). With successive years of participation, this level of use broadens fiscally and programmatically to include more restrictive project grants. Implicit in this general hypothesis is the notion that entry into the system is permanent. Once a community enters the federal aid system, exit is unlikely. Data from the CSA outlays report were used to calculate the amount of project and formula grant monies allocated to new aid recipients in 1972 and 1977.

Table 5 identifies the different modes of participation in the federal aid system between 1967 and 1977. As expected, most communities which entered the federal aid system retain their involvement after entry. Only 72 communities (8.9 percent) deviate from this pattern. In the case of each of these communities, the pattern was to enter the system in 1967, leave in 1972, and re-enter in 1977. No other deviant pattern was observed among the communities studied. Examination of these deviant cases did not identify a trait common to these communities. Standard deviation scores on selected socioeconomic variables indicate that there is substantial diversity among deviant aid participants. More detailed information on the nature of each community's experience with the aid system is necessary before we can arrive at any conclusive statements as to the cause and nature of deviations from the expected participation pattern.

Among those communities entering the federal aid system in 1972 and 1977, a definite pattern of participation can be identified. The mean per capita aid allocation for new participants increased from $4.65 in 1972 to $35.01 in 1977. Both figures lag significantly behind mean allocations for older participants in both years (see Table 2). As expected, the 1977 mean aid allocation for new participants entering in 1977 was lower than the mean allocation for participants who entered the system in 1972. This finding is

Table 6. Mean Per Capita Aid Allocation and Percent of Total Aid Received from Formula-Based Aid Programs for New Aid Participants in 1972 and 1977

	1972		1977	
	Mean Per Capita Allocation	Percent Formula Grants	Mean Per Capita Allocation	Percent Formula Grants
New Aid Participants 1972 (N = 153)	$4.65	65.7	$35.01	52.3
New Aid Participants 1977 (N = 297)	NA	NA	$25.84	58.1

Source: Community Services Administration, *Geographic Distribution of Federal Funds,* Fiscal Years 1972, 1977 (Washington, D.C.: U.S. Government Printing Office).

contrary to Dommel's thesis that new aid recipients under New Federalism received a disproportionately larger amount of federal assistance than did traditional aid recipients.[7] The findings in Tables 2 and 4 demonstrate that new entrants into the federal aid system were significantly needier than previous recipients and that their allocations continue to lag behind less fiscally stressed and older aid recipients.[8]

The scope of participation among new participants confirms the hypothesized pattern of grant use. New participants' initial involvement in the federal aid system is dominated by less restrictive and less costly formula and block grants. For new 1972 aid participants we observed a significant shift from formula grant (i.e., 65.7 percent) to project grant use (i.e., 52.3 percent) in 1977. These findings clearly establish the evolutionary nature of grant use. Reluctant to invest time and money in seeking competitive aid programs, new entrants to the federal aid system seek more secure and less restrictive block grant and revenue-sharing monies. Having experienced the dependence of federal aid, new entrants seek to broaden their use of the federal largesse by seeking and receiving monies from the larger pool of project grants. In fact, federal aid monies from block grants and revenue sharing are used to finance grant-seeking ventures, thus lessening the costs of competitive grantsmanship.

Discussion

Contrary to prevailing views, the expansion of the federal aid system through general revenue sharing and block grants has not lessened the degree of equalization in the allocation of federal aid monies. The evidence points to the opposite conclusion. Those communities which entered the aid system between 1967 and 1977 were the very communities most in need of federal assistance and the object of many aid programs. Moreover, the relationship between need and fiscal capacity is significantly strengthened by the added participation of these new beneficiaries of the federal largesse.

The findings of this study establish an alternative policy for alleviating inequities in the allocation of federal aid. Rather than relying exclusively on a supply-side approach (i.e., alterations in the allocation mechanisms), policy makers might consider the recipients' perspective. Assisting local governments in costly grant applications and encouraging intergovernmental grant applications would help to defray the costs of seeking and receiving federal assistance and thus increase the use of grants and their equalization among needy and fiscally strained communities. Both the block grant programs and general revenue sharing have provided some initial steps in this direction.

Finally, a note of caution. This study has not commented on the substantive policy impact of federal aid programs. Many critics of revenue sharing and block grants claim that these mechanisms fail to achieve their intended policy goals (e.g., increase municipal expenditures in targeted policy areas). Though not unrelated, this issue is beyond the scope of this study. We have attempted to examine only one aspect of the federal assistance program, the allocation process. Impact and evaluation are clearly the next logical steps to be taken in the study of the federal aid system, particularly among new aid recipients.

Appendix:
A Note About Data on Federal Aid Outlays

Studies of the federal aid system are subject to significant obstacles when testing hypotheses. The fundamental problem is the "inability of the nation to keep a usable record of its own fiscal transactions" (Anton, 1978, p. 1). At present there is no single reliable and comprehensive accounting of all federal outlays to state and substate governments.

Information about allocations of federal aid to municipal governments is available from two sources—U.S. Bureau of the Census' *Census of Governments* and the Community Service Administration's (CSA) *Federal Outlays Report*. Anton (1979) identifies three significant shortcomings with the census bureau's data: (1) the functional categories lack specificity, (2) the omission of certain unincorporated areas which are significant participants in the federal aid system, and (3) the exclusion of federal aid monies passed through the state to recipient municipal governments.

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[7] This conclusion requires some qualification. Dommel's analysis was limited to block and formula grants where the grant-seeking costs were not significant for new aid participants. The higher application costs associated with project grants may have limited the mean aid allocation for first-time aid participants.

[8] Some caution should be used in interpreting these findings. The fact that data were available only for five-year periods may distort the characterization of communities as "new entrants" into the federal aid system. An aid recipient new to the 1972 data may in fact have entered the federal aid system at any time between 1968 and 1972. This limitation in the data does not detract from the central hypothesis. The expectation is that cities enter the federal aid system with a heavy reliance on formula aid and block grants, progressively shifting their dependence to project grants. The findings in Table 6 confirm this hypothesis.

CSA outlay data report all federal expenditures to municipal governments over 25,000 in population. *The outlay report includes expenditures well* beyond the scope of federal aid programs (e.g., loans, subsidies, procurements, etc.). Aid allocations are reported by specific programs and administering agencies. Though the CSA data have been collected yearly since 1968, current data holdings are reliable only since 1972 (Anton, 1979).

Some of the problems associated with the census data can be corrected. Aid allocations for unincorporated areas are available from special census reports which can be added to the *Census of Governments* data file. The omission of pass-through monies, though problematic when studying all substate governmental units (i.e., counties, school districts, special districts, etc.), is of minimal importance when studying aid allocations to general purpose municipal governments. The ACIR's analyses (1979, 1980) reveal that most pass-through monies go to special districts (i.e., public school districts) and counties (i.e., general welfare and highways) and not municipal governments.

For studies of aid allocations after 1972, the CSA data are clearly superior to the census bureau's accounting of federal aid transfers. This relative advantage, however, is eliminated for the purposes of this study. The lack of reliable data on aid allocations before 1972 limits the utility of the CSA data in testing the time-series hypotheses posed by this study. Moreover, certain shortcomings identified in the census data set can be corrected, making the census bureau's accounting of federal grant outlays the preferred data base for this study. The work of Thomas Anton's Intergovernmental Fiscal Analysis Project should produce more extensive and reliable data with which to pursue research on federal aid allocations and their impacts on recipients.

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The Election of Blacks to City Councils: Clarifying the Impact of Electoral Arrangements on the Seats/Population Relationship

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The notion that at-large elections for city council seats are discriminatory toward blacks has recently been attacked as empirically invalid. Recent studies have reached conflicting conclusions as to whether electoral arrangements or socioeconomic factors are the major influence on how proportionately blacks are represented. This article addresses this issue, using a regression-based analysis in which proportionality is treated as a relationship across cities with electoral structure as a specifying variable. Socioeconomic variables found to be important in other studies are included. The results support the traditional notion and suggest that the electoral structure begins to have a discernible impact on the level of black representation once the black population reaches 10 percent of the total municipal population. While one socioeconomic variable, the relative income of the city's black population, is found to affect the election of blacks, its impact is greater than that of the electoral structure only when the black population is less than 15 percent.

The notion that at-large elections for city council seats are discriminatory toward black Americans was once conventional wisdom in political science. Given that racially polarized voting patterns are not uncommon in American cities and that blacks are a minority which is usually concentrated within heavily segregated areas of a city, it has often been asserted that black candidates are far more likely to be elected to city councils through single-member district (or ward) electoral systems than through at-large arrangements. Recently, however, this traditional notion has come under serious question. Indeed, the literature now contains a complete range of possible conclusions about the discriminatory impact of at-large elections.

Susan MacManus has argued, for example, that the traditional view is "inaccurate" and "must be abandoned" (1978, pp. 158, 160). In her analysis of data for 243 central cities of Standard Metropolitan Statistical Areas (SMSAs), she finds that electoral arrangements have little or no direct impact on the election of blacks. When she imposed controls for various socioeconomic characteristics of cities, she discovered that the relationship between electoral format and black representation "virtually disappears" (1978, p. 159). Understandably, MacManus concluded that there is a need for "a reevaluation of the relative impact of electoral procedures" (1978, p. 160). Additional support for this revisionist argument is provided in an earlier, more limited study by Leonard Cole, whose examination of 16 New Jersey cities led him to conclude that the traditional notion had been "exaggerated" (1974, p. 26). Like McManus, Cole argues that the socioeconomic characteristics of a city's population are more important determinants of black electoral success. Albert Karnig (1979) has also raised some questions about the relative significance of electoral arrangements. Although Karnig is among those who have marshaled evidence in support of the traditional notion (Karnig, 1976; Karnig and Welch, 1978; see also Kramer, 1971; Jones, 1976), he has more recently argued that although the electoral system is not an unimportant variable in understanding the election of black council members, it is considerably less important than other variables which reflect black political resources, especially the socioeconomic status of the black community (1979, pp. 143-45, 148).

Several other recent studies, however, reach sharply contrasting conclusions. Theodore Robinson and Thomas Dye included various socioeconomic indicators for both the total population and black population in their investigation of councilmanic representation in central cities, yet concluded that the electoral framework is the "single most influential" variable. According to them, "At-large elections, independently of any other socioeconomic or structural variable, significantly reduce black representation on city councils" (1978, p. 139). In a study focusing on only southern cities, Margaret Latimer investigated socioeconomic variables for the black, white, and total populations of cities but still found that it

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We wish to express our appreciation to Theodore P. Robinson and Thomas R. Dye for providing data used in this analysis and to John K. Wildgen for assisting with the preparation of the data.

was the electoral system that had "a preeminent causal effect" on the election of blacks (1979, p. 79). And Delbert Taebel, although not incorporating socioeconomic information into his analysis, has reported that the electoral arrangement is an important factor when blacks constitute between 20 and 40 percent of a city's population (1978, p. 149).

The conflict in these conclusions is of considerable interest for both theoretical and practical reasons. On the theoretical side, the revisionist position suggests that the electoral framework is really little more than a relatively unimportant structural dimension which happens to intervene between more explanatory socioeconomic factors and black representation. The electoral framework had appeared to be one structural variable that would surely survive the onslaught of "environmental determinism," but this now seems to be a matter for debate. On the practical side, the issue is enormously important to those wishing to bring about a more equitable proportion of blacks on city councils, for the electoral structure of a city is certainly more readily changed than the socioeconomic characteristics of a city's population. Changes in electoral structures may be mandated by courts, for example, and blacks have often resorted to litigation in their efforts to substitute single-member districts for at-large elections. Indeed, this is probably one of the litigation issues for which political scientists are most frequently called upon to testify as "expert witnesses" (see, e.g., Davidson, 1979), and many of the studies in this area have been cited in judicial opinions (see, for example, the dissenting opinion of Justice Thurgood Marshall in *City of Mobile* v. *Bolden,* 48 LW 4436, at 4449-4450, n. 3 [1980]).

Given both the theoretical and practical import of this issue, it seems especially important to clarify the impact of electoral arrangements on the election of blacks to city councils. In this article we employ an analytic design which we believe to be more appropriate, and even more powerful, than those used previously. This alternative approach, which avoids some significant methodological problems and conflicts present in the other studies, allows us to demonstrate more clearly the kind and degree of impact that different electoral arrangements and socioeconomic factors have on the election of blacks.

An Alternative Strategy

The disparity in conclusions across the various studies may be partly a function of different methodological choices. Two dimensions in particular—the measurement of the dependent variable and the criteria adopted for selecting the cities to be included in the analysis—may contribute to the inconsistent results. The dependent variable has in all cases been a measure of what Hanna Pitkin calls "descriptive representation," the correspondence between the percentage of a city's population which is black and the percentage of seats on a city's council held by black people (1967, pp. 60-91). Two different operationalizations have been employed, however. The measure most frequently used has been a ratio, i.e., the council percentage divided by the population percentage. None of the studies using this measure has concluded that the electoral system is an unimportant variable. The revisionist findings of both MacManus and Cole, however, are based on a different operation. They (as well as Taebel, who did not control for socioeconomic factors) calculate the *difference* between these percentages, i.e., population percentage subtracted from council percentage. Objections may be raised to both measures. The ratio has the undesirable consequence of scoring every city without a black person on its council as zero, regardless of the proportion of its population that is black. The subtractive measure avoids this particular problem, but will also produce quantitative equivalencies in cases that many analysts may feel to be qualitatively quite different. In particular, one city's score on this index may register the maximum degree of underrepresentation possible for that city (e.g., council percentage = 0; population percentage = 10), yet be identical to the score for another city in which the underrepresentation may be quite far removed from its theoretical maximum (e.g., council percentage = 35; population percentage = 45).

This latter consideration introduces another methodological issue: the criteria for determining which cities will be included in the analysis. All but MacManus and Latimer have established some black population threshold as a requirement for inclusion. The rationale, of course, is that there must be at least a minimum level of black population before the black community can have a realistic chance of electing one of its own to the council under any electoral arrangement. There has been no consensus on what that threshold should be, however. Analysts appear to have made little more than intuitive judgments on this matter, judgments that may reflect concern for sample size as well as minority population. Most have adopted a fixed percentage, requiring the black population to be at least 5, 10, or 15 percent of the total population. The only exception to the fixed percentage has been Taebel's requirement that the black population be equal to at least one-half of the population of a single-member district, assuming the entire council is elected from districts (1978, p. 144).[1]

These methodological considerations undoubtedly influence what one discovers about the effects of different electoral systems. Unfortunately, there are no clearly objective nor widely agreed-upon guidelines for making these determinations, and the interpretation of findings must consequently be conditional to the research strategies adopted. Fortunately, however, a slight shift in approach provides another, more appropriate way to address this topic, one which also circumvents these difficult methodological choices.

There is a distinct similarity between these studies of descriptive racial representation and studies which examine the translation of votes for a political party's candidates into seats in a legislative body for that party. Both are primarily concerned with proportionality, in one case between population and seats, in the other, votes and seats. In the partisan studies, however, proportionality has been treated as a *relationship* between two variables across elections, rather than as a variable itself (e.g., Tufte, 1973). If this same approach is applied to the data on racial correspondence—treating proportionality as a relationship across cities rather than as a dependent variable—then the measurement and threshold issues discussed above virtually disappear. The proportion of council members who are black may simply be regressed onto the black proportion of the population. The resulting equation will provide an estimated slope coefficient, or "swing ratio" (Tufte, 1973, p. 542), for this relationship. As long as the intercept for the regression equation is fairly close to zero, this coefficient may be interpreted as a measure of proportionality (see, e.g., Rae, 1971, pp. 89-90). If the slope coefficient has a value of 1.0, the relationship is directly proportional. A slope greater than 1.0 would mean the proportion of blacks on city councils generally exceeds the proportion of blacks in the population, while a slope of less than 1.0 would mean the council proportion is generally lower than the population proportion.

There are definite advantages to addressing the substantive issue in this fashion. Not only does this obviate the measurement debate over division or subtraction, it avoids the major problems associated with each of these options as well. Under this approach, proportionality is a relationship across a set of data points, each of which reflects the specific black proportions of the population and the council for a city. The fact that all cities without a black council member do not have the same black population percentage is taken into account in estimating this relationship. All cities without a black council member are not, therefore, treated as empirically equivalent in this procedure, as is the case with the ratio measure. Likewise, all cities with the same deviation from proportional representation are not treated as empirically equivalent, as is the case with the subtractive measure. This procedure is sensitive to the fact that cities with identical or similar deviations may have widely varying percentages of blacks in their populations. Treating proportionality as a relationship across cities also obviates the need for a threshold. The use of a threshold is based on an assumption that only when the black population of a city exceeds a certain level can blacks expect to hold a seat on the council. In a regression-based analysis, this a priori assumption may be treated as an empirical question. If a threshold is indeed discovered, it may be taken into account by estimating the appropriate nonlinear swing ratio. In addition, if the electoral format is an important variable, this approach will allow one to estimate empirically the point at which structure actually begins to make a difference in how proportionately blacks are represented.

A regression-based analysis also permits one to examine the impact of electoral arrangements in the conceptually most appropriate way, as a *specifying variable* which establishes conditions under which the seats/population relationship varies, rather than as an independent variable with a direct impact. The traditional theoretical argument asserts that the major factor affecting the election of black council members is the relative size of the black electorate (although, due to the absence of voter registration figures by race in all but a few cities, the actual meaure has been the black proportion of the population). The influence of this variable, however, is viewed as conditional. Its influence is expected to vary across different electoral structures, having a much greater impact within the district format than in at-large arrangements. This may be tested by inserting electoral structure into the regression equation as a series of dummy variables that interact with the black population percentages. If the traditional notion is correct, the slopes for the seats/population relationship should be quite different across the election types, and should remain so when socioeconomic factors are also included in the equation.

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[1] The threshold issue is related to the measurement issue in that the lower one sets the threshold requirement, the more important the difference in measurements becomes. It has been suggested that whether the ratio or subtractive measure is adopted is empirically unimportant, because essentially the same relationships appear regardless of which measure is used (Karnig and Welch, 1979, p. 466; and 1978). This is likely to be true, however, only if the threshold is set sufficiently high. Based on the Robinson-Dye data, the correlation between the two measures is only .45 across all cities (no threshold). Among cities that are at least 5 percent black in population, the correlation increases to .72. When the threshold is set at 10 percent or 15 percent, it increases to .83 and .92 respectively. Clearly, if one is willing to adopt one of the higher thresholds, the measurement issue is largely moot. If theoretical considerations or concerns for sample size dictate that either no threshold or a low threshold be adopted, however, the measurement issue is quite serious.

The results of a regression-based analysis of this type are reported below. The data are taken from the *Population-Policy Data Base: U.S. Cities and Suburbs, Standard Metropolitan Statistical Areas* (PPDB), compiled under the direction of Thomas R. Dye. The information on electoral structure and number of black council members was collected during October 1976, by means of a telephone survey of city attorneys and clerks in the largest city of each SMSA (N=243). All population figures and socioeconomic indicators were derived from the 1970 Census of Population. Because we are interested in the impact of different electoral structures on the black *minority,* the following analysis is based on the 239 central cities in which blacks constitute less than half of the total population.

Impact of Electoral Format and Socioeconomic Factors

Before comparing the impact of electoral structures and socioeconomic factors, we need to estimate the simple bivariate relationship between the black percentage of the population and the black percentage of the council. The data for the 239 cities result in the following regression equation:

$$BCC\% = -.498 + \underset{(.041)}{.593}BP\% \text{ with } \overline{R}^2 = .471$$

where $BCC\%$ is the black percentage of the council and $BP\%$ is the black percentage of the population. The intercept, —.498, is quite close to zero allowing us to treat the slope as a measure of proportionality. Using this equation, one can generally expect blacks to be "underrepresented" on city councils, with the amount of that underrepresentation becoming more acute as the black proportion of the population increases. The percentage of seats on a city council held by blacks tends to be only about .60 of the black percentage of the population.[2]

Electoral Format. The existing literature on the election of blacks to city councils has been concerned, first and foremost, with the possible impact of different electoral arrangements. These have traditionally been classified into three groups: systems that elect all of the council members at-large, systems electing all members from districts, or mixed systems which use some combination of at-large and district elections. It has been argued that this classification scheme is not sufficiently discriminating, that at-large elections with place systems and/or district residency requirements should be distinguished from the "pure" at-large situation (MacManus, 1978). Although these differences are present across at-large elections, their conceptual linkage to the issue of vote dilution may be quite tenuous. Ultimate control over the outcome in these modified arrangements still remains with a *citywide* electorate (see Davidson, 1979, p. 337).[3] In addition, this more extensive classification may in some cases produce an empirical problem—the number of observations for each of the modified forms may be too limited for reliable analysis. For example, in the McManus study, based on 243 cities, the Ns for the different categories of the modified types were only 6, 7, and 16. Given these problems with the more extensive classification, we have adopted the traditional trichotomy for this analysis. This results in 128 at-large systems, 36 districted systems, and 75 mixed arrangements.

When the electoral format is introduced into the regression equation as dummy variables and interaction terms, the different types of elections have a rather dramatic impact on proportionality (Figure 1). The slope coefficient for districted cities is virtually equal to 1.0, whereas the slope for at-large systems is only about .5. The impact of mixed systems, not surprisingly, falls between that of the other two. Clearly, a black minority is more likely to attain proportional representation through an electoral system that does not have any at-large elections.[4]

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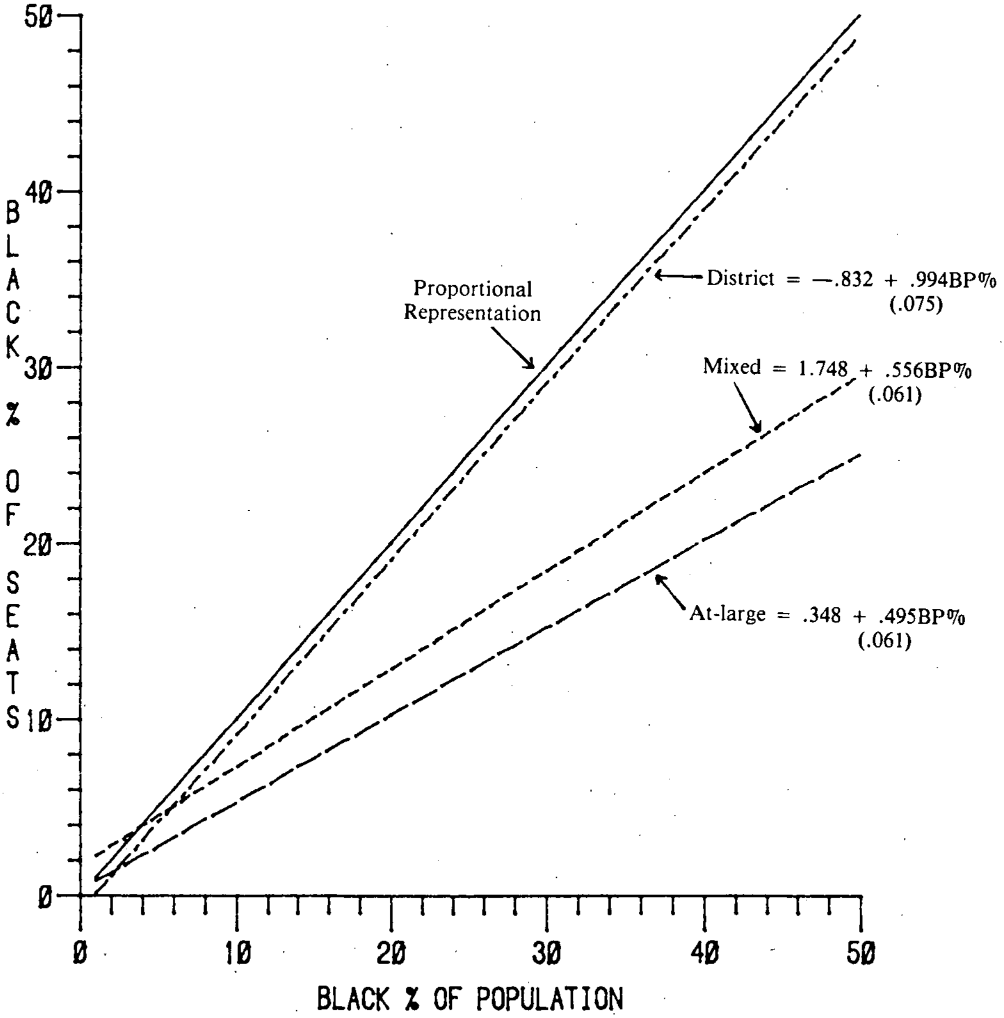
[2] It was suggested above that the notion of a threshold could be empirically tested through nonlinear regression. Logit transformation indicates that a linear fit is quite appropriate in this case, however, as the slope for the $\log_e$ of the population odds is .829.

[3] Residency requirements do result in council members whose residencies are geographically dispersed. Given control over the ultimate selection by the citywide electorate, however, this requirement does not assure that blacks will be elected to the council except in the extremely unlikely situation that all possible candidates in a "residency district" are black. If the city electorate is polarized along racial lines, the white majority may easily select a white candidate from a majority black residential district. Evidence from several studies suggest, in fact, that such requirements have not resulted in more proportionate black representation (see Karnig and Welch, 1978, p. 15; Robinson and Dye, 1978, p. 137; and MacManus, 1978, p. 157).

Figure 1 not only shows that there is a tendency for blacks to be less proportionately represented in at-large systems than in districted systems, it also shows that the electoral structure begins to make a difference even when blacks constitute a fairly small percentage of the population. In order to estimate the point at which the electoral format can be expected to have an impact, we have added 90-percent confidence bands around the regression lines for both the districted and at-large cities. These bands are displayed in Figure 2, which shows that there is no overlap between them beginning at a point just below 10 percent. Thus, as a conservative estimate, one could con-



Source: Compiled from Population-Policy Data Base, Florida State University, and 1970 Census of Population.

Note: The overall adjusted R^2 is .540 and the within group adjusted R^2s, which legitimately can be compared given the similar standard deviations for the black population percentages (for districted, 13.7; mixed, 12.3; at-large, 13.2), are .816 for districted cities, .510 for mixed cities, and .344 for at-large cities.

Figure 1. Relationship between Black Percentage of Population and Black Percentage of City Council in Different Electoral Formats

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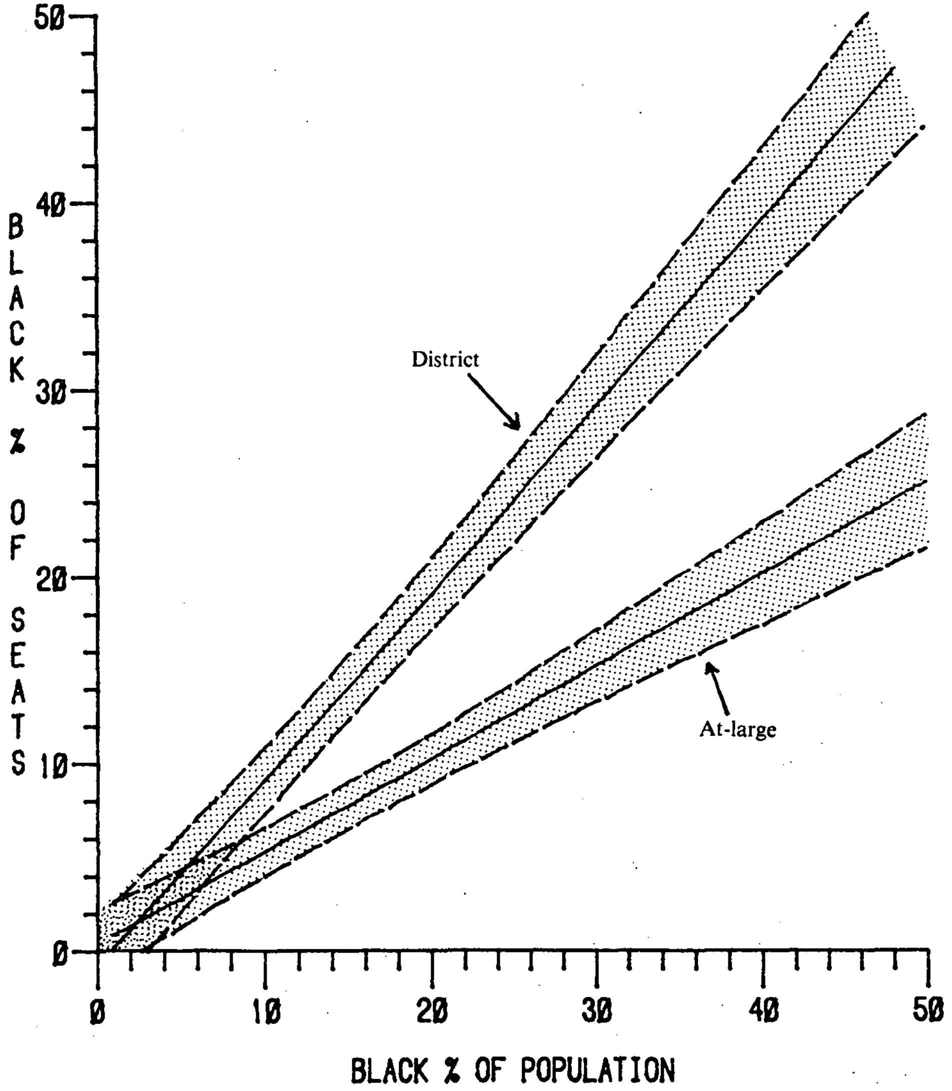
[4] Tests for nonlinear fits show that the reported linear fits within all three categories are quite appropriate.

sider 10 percent to be a threshold or critical point at which at-large elections can be expected to have an adverse impact on black representation.[5]

Socioeconomic Characteristics. While the above analysis certainly supports the traditional notion concerning the effect of different electoral structures, the question remains whether that inference will withstand controls for various socioeconomic factors. MacManus (1978) found that there was no relationship between electoral format and black representation when controls were imposed for the size, growth rate, and income, educa-



Source: Compiled from Population-Policy Data Base, Florida State University, and 1970 Census of Population.

Figure 2. Estimated Seats/Population Relationships with 90-Percent Confidence Bands in Districted and At-Large Electoral Formats

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[5] Although Taebel focuses on the 20 percent figure as a "tip-point," electoral arrangements clearly begin to have an impact in his analysis when the black population exceeds 10 percent (1978, p. 150).

tional, and occupational characteristics of a city's population. Cole (1974) also focused on income, educational, and occupational characteristics in arguing that electoral arrangements have only a minimal impact.

In order to test this revisionist argument, we have added to the regression equation the five variables for which MacManus controlled: population size (in ten thousands), rate of population change from 1960 to 1970, median family income, median school years completed by those over 25 years of age, and the percentage of the labor force employed in white-collar occupations. The results, reported in Table 1, do not support the alternative argument. The slopes for the seats/population relationship across the different electoral formats are affected only minimally, while the independent effect of these additional variables on the percentage of blacks on a city council is negligible when the black population percentage is also in the equation. It might be argued, however, that it is more reasonable to expect these environmental factors to have a conditional rather than uniform impact. In districted cities, the black population percentage may virtually be all that matters in explaining the level of black representation, while in cities with at-large elections the population characteristics may have a discernible effect. This does not appear to be the case either, however. Table 1 also contains the results of the regression analysis in which these environmental variables are entered as interaction terms. The slopes for the three seats/population relationships are again virtually unaffected, and the regression coefficients for the socioeconomic factors continue to suggest only minimal impact, if any.

While our findings clearly contradict those of MacManus (and Cole), it may be possible to reconcile the conflict. MacManus suggests that proportional racial representation is more likely to be found in "smaller cities, characterized by growth, greater wealth, and more highly educated populations" (1978, p. 160). These are also the types of cities that are more likely to have relatively small percentages of blacks in their populations. For example, the 25 cities in this analysis which are under 100,000 and above the mean on growth, income, and education have an average black population percentage of only 4.3, compared to 15.8 for the remaining cities. When the black percen-

Table 1. Seats/Population Relationships under Different Electoral Formats with Uniform and Conditional Impacts of Socioeconomic Characteristics

	Uniform Impact[a]	Conditional Impact[b]		
		Districted	Mixed	At-Large
Intercept	–	–.920	–10.208	11.630
Percent Black Population (Districts)	1.010 (.096)[c]	1.033 (.086)	–	–
Percent Black Population (Mixed)	.556 (.078)	–	.598 (.080)	–
Percent Black Population (At-large)	.512 (.054)	–	–	.474 (.062)
Total Population (10,000)	.000 (.131)	–.008 (.034)	–.003 (.024)	.110 (.037)
Population Growth Rate	.023 (.019)	.012 (.040)	.019 (.025)	.031 (.033)
Median Family Income	.000 (.001)	–.001 (.001)	.000 (.001)	.000 (.001)
Median School Years	–.471 (.930)	4.877 (2.676)	1.391 (1.588)	–1.472 (1.269)
Percentage White-Collar	.010 (.099)	–.130 (.348)	–.114 (.162)	–.038 (.151)

Source: Compiled from Population-Policy Data Base, Florida State University, and 1970 Census of Population.

[a] The slopes for the population percentage variable in the mixed and at-large cities are the actual estimates of the seats/population relation for these groups. The intercepts for districted, mixed and at-large cities are .956, 3.667, and 2.307, respectively, and the adjusted R^2 is .534.

[b] The overall adjusted R^2 is .545 and the within group adjusted R^2 s are .842 for districted cities, .506 for mixed cities, and .405 for at-large cities.

[c] The standard errors of the reported slopes are provided in parentheses.

tage of a city's population is small, the subtractive measure can register no more than a minimal level of underrepresentation, even if the council is all white. It may therefore be possible to restate MacManus' conclusion to hold that blacks are "better" represented in these cities not because they are small, growing cities with relatively well-educated and wealthy populations, but because there simply are not very many blacks in these cities and, given the subtractive measure, they are therefore fairly well represented by definition. MacManus' overall findings regarding the impact of environmental factors may also simply be reflecting the fact that there is a strong negative association between the black population percentage and the subtractive measure (r = —.51, using the data for the 239 cities in this analysis). Indeed, when we examine the five bivariate relationships between the environmental indicators and MacManus' subtractive measure of proportionality, controlling each time for the black population percentage, the partial correlations range from .0 for occupation to .10 for income. It is highly questionable whether these variables really have an independent impact of their own, separate from their association with the black population percentage.

It has been argued, however, that the relevant socioeconomic indicators are not those for the entire population of a city, but those for the black community within a city. Noting that an expanded black middle class means more blacks with "skills, time, and money for effective electoral politics," Karnig argues, "As blacks possess private-sector resources more competitive with those of whites, they are more likely to obtain reasonably proportional public-sector representation" (1979, p. 145). In Karnig's analysis, the variable which most influenced how proportionately blacks were elected was the black/white income differential; the closer black median income approached white median income, the more closely the percentage of seats held by blacks approached the black population percentage.

Consistent with the strategy employed thus far, we have added an income differential variable (the ratio of black median income to the median income for the entire city) to the regression equation used in Figure 1. (Because the census does not report the black median income for 15 of the cities, all of which have black population percentages less than 1.0, the following analysis is based on 224 rather than 239 cities.) Once again, the electoral format is of major importance. The slopes for the seats/population relation within each category of electoral structure remain virtually unchanged (districted = .999 [s_b = .103]; mixed = .555 [.075]; at-large = .501 [.055]). The regression coefficient for the income ratio is a not unimpressive 8.218, but its standard error (4.820) suggests that the estimate is not very reliable.

It may be more reasonable to expect a conditional rather than uniform impact for the income differential variable as well. Effective citywide campaigns are generally assumed to be more expensive and to require more extensive organizational support than district-based campaigns, and the relative "skills, time, and money" reflected by this variable may have the greatest impact, therefore, in at-large settings. To estimate the conditional effects, we have added this variable to the regression equation as a set of interaction terms. The results, which are quite intriguing, are reported in Table 2. The impact of the electoral structure on how proportionately blacks are represented remains, but a conditional impact for the income ratio is also apparent. The seats/population relationships within each of the electoral format categories are again virtually unchanged, although the additional variable does have the effect of separating out the intercepts for the three groups. The regession coefficients for the income ratio, however, do show a conditional impact on the black council percentage. The coefficient for the at-large setting is a very impressive 17.583, while that for the districted cities is —20.153, although this last value is not very reliably estimated (s_b = 12.142).

The information contained in Table 2 suggests that both electoral format and the income ratio have an effect on the level of black representation. This is consistent with Karnig's findings, although he further argues, based on a comparison of standardized regression coefficients, that the relative income of the black population has a *greater* effect than election structure on how proportionately blacks are represented. There is a problem, however, in trying to discuss standard deviation unit changes (the basic metric for the standardized coefficients) in a variable such as election format. It is difficult to imagine changing the electoral format by one standard deviation; these changes are, rather, from one structural type to another. An alternative way of making this comparison, which avoids this problem, is to estimate the expected consequence of changing the value of one variable while holding the other constant. When this is done, we discover that the relative impact of the two variables can be expected to differ dramatically, depending on the percentage of blacks in the city's population.

Figure 3 illustrates the different impacts of the two variables. The solid line serves as a baseline, and represents the relation between population and seats for cities with (1) at-large elections and (2) an average income ratio for at-large cities (.673). From this baseline the expected consequence of changing to district elections while

Table 2. Seats/Population Relationships under Different Electoral Formats and Conditional Impact of Income Ratio

	Districted	Mixed	At-Large
Intercept	12.690	−2.473	−11.733
Black Population Percentage	1.009 (.081)	.554 (.063)	.503 (.061)
Income Differential	−20.153 (12.142)	6.180 (7.257)	17.583 (5.802)

Source: Compiled from Population-Policy Data Base, Florida State University, and 1970 Census of Population.

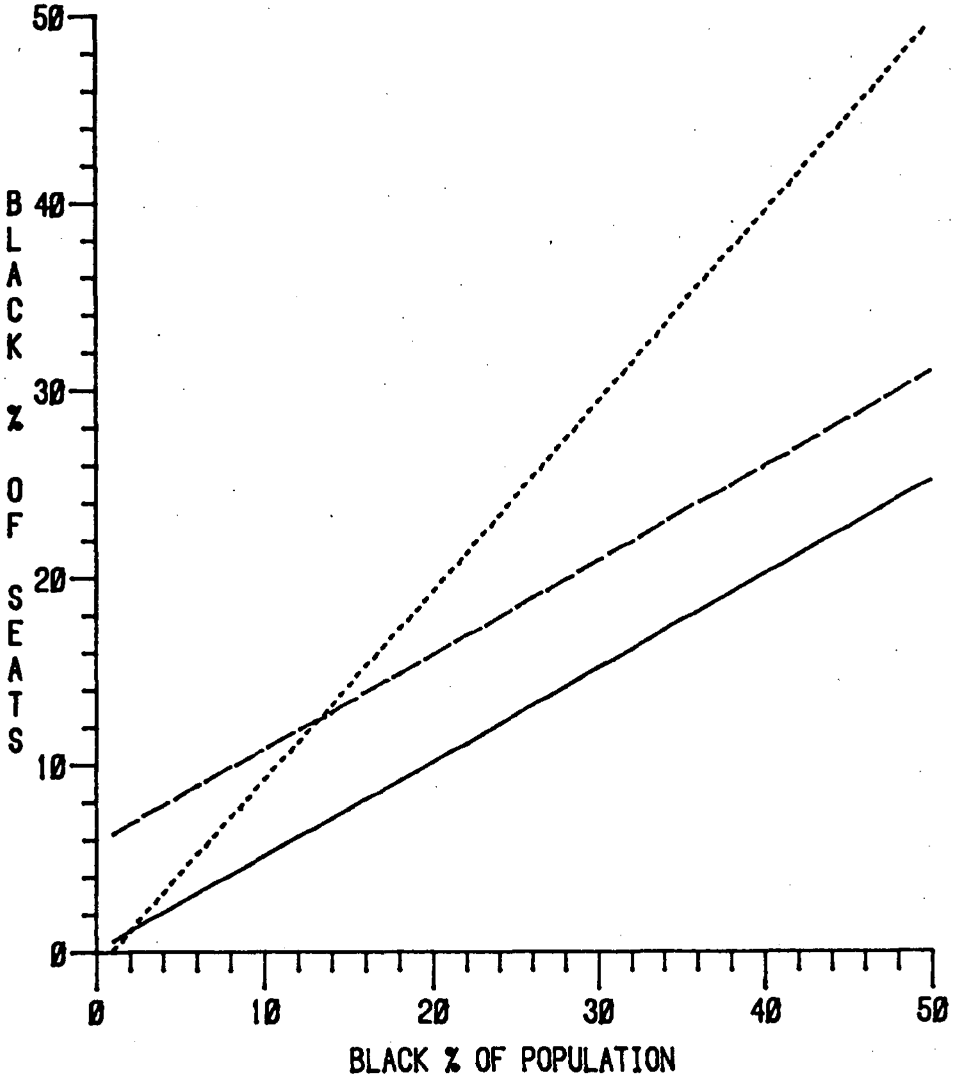
Note: The slope and intercept estimates are for the impacts of their respective variables within the particular electoral format groupings. The standard errors of the reported slope coefficients are in parentheses immediately below each term.

maintaining an average income ratio (the dotted line) can be compared to the expected consequence of increasing the income ratio to 1.0, an increase of almost three standard deviations above the mean, while retaining the at-large arrangement (the broken line). It is apparent from Figure 3 that both the structural and income changes increase the expected percentage of black council members across the entire range of black population percentages, but the relative implications of these changes are dependent upon that population percentage. At lower population percentages, achieving income equality is expected to add more than will a change in the electoral format, but at higher percentages changing the electoral format will have a much greater impact. For example, in cities where blacks comprise only 10 percent of the population, increasing the income ratio from .673 to 1.0 would be expected to increase the black percentage of the council by 5.9, compared to an expected increase of only 1.6 if the electoral structure were the variable that changed. If the black population percentage were 50, however, increasing to income equality would again be expected to add 5.9 to the council percentage, whereas a change to election by districts would be expected to add 24.3. From the data reflected in Figure 3, one could conservatively estimate that once the black population percentage reaches about 15, the electoral format has more impact than the relative income of the two racial groups.

Conclusion

The evidence reported above strongly supports the proposition that at-large elections, at least in comparison with district-based electoral systems, tend to "under-represent" black people. Districted systems may themselves be gerrymandered to reduce the level of black representation, of course (see, e.g., Engstrom and Wildgen, 1977), but as a general matter the black community can expect more proportionate representation through these systems than if at-large elections are used. From this analysis, we can conservatively estimate that the electoral format begins to make a difference when the black population constitutes around 10 percent of a city's population, and that the underrepresentation occurring in at-large cities becomes more acute as the black population proportion increases. This relationship between the electoral structure and black representation was found to be unaffected when various socioeconomic factors that have previously been suggested as important determinants of black representation were also investigated, although one socioeconomic variable—the relative income of the black population—did have an important impact on the percentage of black council members. This variable, in fact, appears to be a more influential determinant of black representation than is electoral format when the black population of a city is relatively small, but when blacks begin to constitute a fairly substantial minority of a city, then the electoral structure begins to be the more influential determinant. From the above analysis, we can estimate, again conservatively, that once the black population reaches about 15 percent of the total population, the electoral structure will have the greater influence.

The notion that differential electoral frameworks seriously affect how proportionately blacks are likely to be represented on city councils should not, as some have suggested, be abandoned. The evidence reported above strongly supports the proposition that, in comparison with single-member districts, at-large elections seriously reduce the level of black representation. This is not a novel conclusion, but it remains an important one, because at-large elections are a prevalent, yet alterable, structural dimension in American municipalities.



Source: Compiled from Population-Policy Data Base, Florida State University, and 1970 Census of Population.

Key: ———— Cities with at-large elections and average income ratio.
– – – – – Cities with district elections and average income ratio.
- - - - Cities with at-large elections and income equality.

Figure 3. Comparing the Impacts of Changes in Election Format and Relative Black Income on the Election of Blacks to City Councils

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Personal Attribute Models of the Voting Behavior of U.S. Supreme Court Justices: Liberalism in Civil Liberties and Economics Decisions, 1946-1978

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The prevailing view among students of judicial politics is that judges' background characteristics or personal attributes cannot provide satisfactory explanations for variation in their decision-making behavior. Parsimonious attribute models reported here account for 70 to 90 percent of the variance in the voting of postwar Supreme Court justices in split decisions concerning civil rights and liberties, and economics. Seven variables representing six meaningful and easily interpretable concepts achieve this success. The concepts are Judge's Party Identification, Appointing President, Prestige of Prelaw Education *(economics only),* Appointed from Elective Office, Appointment Region *(civil liberties only),* Extensiveness of Judicial Experience, *and* Type of Prosecutorial Experience. *The impressive performance of these models is attributed to superior measurement, operationalization, and model building; to a greater similarity between personal attribute models and more fully specified ones than has been assumed; and to the possibility that the attitudes which intervene between the personal attributes and the voting of judges are causally very closely linked to voting.*

There is a substantial consensus among most prominent students of judicial politics that the background characteristics or personal attributes of judges cannot provide satisfactory explanations of their decision-making behavior. Goldman and Sarat state this consensus well (1978, p. 374):

> In general, the background-behavior studies of aggregates of judges have not satisfactorily established clear-cut linkages between most background variables and decision-making. The reason for this is that taken literally, the sociological model of decision-making does not make sense. . . . What is essentially being argued is that the fact that one has certain attributes means that one will have had certain socializing experiences that have stimulated the development of certain attitudes and values or even conceptions of the judicial role. But in reality each of the background or attribute variables is too crude to be associated with the same or similar experiences; hence each of the variables is not easily linked to just one set of attitudes and values. There are some weak-to-moderate associations between a few attributes and certain types of behaviors; however, the background-behavior model has not been conclusively proven, and for the reasons just suggested it is unlikely that it can be.

Very similar arguments have been put forward by Schubert (1964, p. 446), Grossman (1966, p. 1562), Becker (1970, p. 79), and Murphy and Tanenhaus (1972, pp. 107-09).

These scholars agree that personal attributes have limited explanatory potential because they are only indirect causes of judicial decision making. A variety of important attitudinal variables intervene (see Figure 1, below). It is these intervening variables that should account for most of the variation in decision behavior.

Despite the persuasiveness of the above arguments, it appears that scholars have assumed what Ulmer (1973b, p. 627) called the "risk of premature closure" in rejecting attempts to explain the voting behavior of Supreme Court justices through the use of judges' background characteristics or personal attributes. This article reports parsimonious attribute models which explain 70 to 90 percent of the variance in the justices' liberalism in the two case areas, civil rights and liberties and economics, which have received paramount attention from the modern Supreme Court. It demonstrates that satisfactory "attribute" models of judges' decision-making behavior can be developed, at least for the justices of the United States Supreme Court.

The data analyzed in this article accompany Ryan and Tate (1980). The data for the 1946-69 terms were originally collected by Glendon Schubert; those for the 1969-72 terms by Richard E. Johnston; those for the 1973-77 terms by Andrew Van Esso under the supervision of John Ryan. The Schubert data were supplied by the Inter-University Consortium for Political and Social Research. Financial support has been provided by the National Science Foundation through APSA and by the American Judicature Society. I am grateful to all these individuals and agencies for their assistance. However, none of them bears any responsibility for the analyses reported here.

The Analysis

Justices. Limitations on available, comparable decision data restrict this analysis to the 25 justices serving on the Supreme Court from the beginning of the 1946-47 term to the end of the 1977-78 term. The group includes all but two of Franklin Roosevelt's seven appointees and the appointees of subsequent presidents through Gerald Ford.

Decision Behaviors. In the period since the end of World War II, the United States Supreme Court has devoted most of its efforts to resolving cases concerning two broad substantive issues: civil rights and liberties, and economics. Over two-thirds of all decisions rendered since 1946 have been unambiguously classifiable into these two categories. No other issue accounted for as much as one-sixth of all decisions. (See Ryan and Tate, 1975, p. 11.) Almost every case decided in these two categories can be classified as having a "liberal" or "conservative" outcome, depending upon whether the Court's majority favored the claims put forward by civil liberties claimants or economic "underdogs." Similarly, the votes of the individual justices can be classified as liberal or conservative according to whether they favored or opposed the legal demands of civil liberties claimants and economic underdogs.

Such classification is by now routine in studies of judicial decision making. Specifically,

> in a civil liberties case, justices who were in favor of granting (or granting more of) the claimed civil right were classified as supporting the *liberal* position; justices who were opposed to granting (or who wanted to grant less of) the claimed right were classified as supporting the *conservative* position. . . . *Liberals* in economics cases support unions over management, government regulation of business activities, workers claims against employers, and small businessmen over large corporations. *Conservatives,* then, do the opposite (Ryan and Tate, 1975, p. 13).

Cases which could not be clearly classified as either civil rights and liberties or economics cases were excluded from the analysis, as were civil rights and liberties and economics cases in which the outcome could not be classified as liberal or conservative.

The availability of legally respectable liberal and conservative positions to individual justices depends upon the Court's decision being non-unanimous, which is usually the case. For this reason, the decision data analyzed here are drawn from the justices' voting in all split decisions in civil rights and liberties and economics cases reported from October 1946 through September 1978 (N=2327). From these data, two simple measures of Supreme Court justice decision behavior have been calculated:

1. Percent liberal of all civil rights and liberties case votes cast by the justice (%LIBCL);
2. Percent liberal of all economics case votes cast by the justice (%LIBECON).

The number of cases falling into the substantive areas and summary statistics for these dependent variables and their relationship are reported in Table 1.

The use of these percentages deserves some discussion. Since they do not represent judicial responses to identical sets of case stimuli, comparing them requires the assumption that each justice's particular set of, say, civil rights and liberties cases constitutes an essentially random sample of all possible civil rights and liberties cases to which he could have responded. Under this assumption, each justice's proportion of liberal votes in the sample cases is an unbiased estimate of the proportion of liberal votes he would have cast in the universe of all possible civil rights and liberties cases. Such estimates are subject to sampling error, of course, but are directly comparable.

Clearly it is not possible to demonstrate unequivocally the validity—or invalidity—of the above assumption. I will compare Supreme Court justices' voting percentages because the percentages are superior to any other measurement of the voting ideologies of the justices which might have been appropriate for this analysis. The percentages are simple, robust measures of the behavior of the justices across their entire careers. They constitute macro-level measures of the central tendencies of the justices' voting. In addition, the percentages appear to have strong face validity as measures of what scholars and lay observers of the Court mean when they label particular justices "liberals," "conservatives," or "moderates."

There are several reasons for using these percentages to compare the behavior of justices with non-overlapping or only partially overlapping careers. Such a practice is consistent with the literature on the relation between judges' characteristics and judicial decision making. Bowen (1965), Johnston (1976), Lindstrom (1968), Rowland and Carp (1978), Ulmer (1973), and Vines (1964) all used percentage measures to compare the voting of judges deciding different sets of cases, while Goldman (1966, 1975) used a simple linear transformation of a percentage score incorporating a middle classification position for partial support of the liberal position. In fact, of the backgrounds-decisions studies surveyed for this study, only those of Nagel (1961, 1974) used different measurements of dependent decision be-

Table 1. Liberalism in Voting on Civil Rights and Liberties and Economics Decisions in the U.S. Supreme Court, 1946-1978

	Civil Rights and Liberties Cases (% LIBCL)		Economics Cases (% LIBECON)	
Justice	Votes (N)	% Liberal	Votes (N)	% Liberal
Black	1044	73.3	740	85.4
Reed	282	11.0	305	45.2
Frankfurter	487	46.6	452	21.0
Douglas	1230	93.4	772	82.1
Murphy	87	94.3	110	96.4
Jackson	218	36.7	208	13.9
Rutledge	89	80.9	113	96.5
Burton	367	20.4	373	30.3
Vinson	208	17.3	217	41.5
Clark	685	25.3	532	65.2
Minton	181	16.0	177	52.0
Warren	743	79.1	510	82.0
Harlan	795	23.3	477	23.3
Brennan	1120	81.4	553	74.5
Whittaker	195	26.3	169	17.2
Stewart	1038	45.8	464	35.6
White	884	41.5	346	58.4
Goldberg	167	89.2	100	66.0
Fortas	248	84.3	102	67.6
Marshall	569	84.2	154	67.5
Burger	450	16.2	120	20.0
Blackmun	420	26.0	109	32.1
Powell	351	30.8	93	36.6
Rehnquist	352	4.5	96	15.6
Stevens	148	54.7	39	48.7
	Cases (N)		Cases (N)	
COURT	1452	50.6	875	60.5
Mean (25 Justices)		48.1		50.9
Standard Deviation		30.2		26.3
Correlation (r) with % LIBCL		1.000		.7972

Source: Computed from data accompanying John Paul Ryan and C. Neal Tate, *The Supreme Court in American Politics: Policy Through Law,* 2nd ed., Washington, D.C.: American Political Science Association, 1980.

havior. Even so, Nagel's measure still required assuming that the sets of decisions heard by different courts constituted essentially random samples of all the decisions which could have been heard by the courts concerning a particular issue.

Not only are the percentage measures consistent with the prevailing practice in the literature, they also are highly correlated with a variety of alternative measures used in comparing the justices' behaviors. Thus the results obtained using them would converge strongly with results obtained using any of these alternative measures. For example, the simple correlations between the percentage measures reported in Table 1 and Schubert's "composite scale rankings" for the 20 justices analyzed both here and in his *The Judicial Mind Revisited* (1974, p. 60) were —.94 for civil rights and liberties percentages-C scale rankings and —.88 for economics percentages-E scale rankings. The composite scale scores were derived from Schubert's analysis of a series of "dominance matrices" which sought to determine the relative ordering the 20 justices *would have exhibited* had they all voted on the same C and E scale cases.

If one analyzes like correlations for the groups of 9 justices who actually sat together and decided essentially the same cases in the 10 "natural courts" covered by Schubert's analysis, one finds a similar result. The mean correlations between the *overall* voting percentages reported in Table 1 and the justices' ranks on Schubert's C and E scales for the 10 natural courts are —.89 for the civil rights and liberties percentages-C scale rank scores, and —.92 for the economics percentages-E scale rank scores.

The percentage liberal scores used here correlate highly with the C scale and E scale ranks of the 10 different groups of Supreme Court justices represented in Schubert's natural courts. This indicates that a justice's voting record across his entire career is an acceptable estimate of his voting at different times during that career, despite differences in cases and colleagues. Beyond this it is not possible to go. One can never demonstrate conclusively that a justice with a conservative voting record during the late 1960s and 1970s might not have been a moderate or even a liberal had he encountered the cases and colleagues of the late 1940s and early 1950s. To require such a demonstration is simply to prohibit multivariate analysis of the behavior of Supreme Court justices, since one will never be able to be sure of the relative liberalism or conservatism of more than the 9 justices serving at any one time.[1]

Independent Variables. The independent attribute variables used here to predict Supreme Court justices voting behavior were selected according to availability and use in previous research. Information on the justices' personal attributes was coded from readily available sources such as biographical directories (e.g., *Who's Who in America*) and the data collections of scholars such as Schmidhauser (1959), Krislov (1972), and Abraham (1974, 1975).

All significant attributes which theory or previous research suggest as relevant to Supreme Court decision making can be operationalized from available data. As far as possible, *all* characteristics found to be potentially important in previous research are investigated. Indeed, because the treatment of some characteristics has varied across previous studies, several version of some traits (age, for example) have been created for consideration for inclusion in the prediction equations. Table 2 summarizes the attributes investigated.

The attributes are divided into four major categories. The first category includes traits relating to the birth, upbringing, and education of the justices.[2] Previous research suggests that, among these traits, religion should be expected to have the greatest impact on the justices' voting behavior in civil liberties and economic cases, and that the magnitude of its impact should be greater for civil liberties than for economics (see Ulmer, 1973b; also see Nagel, 1974; Goldman, 1975; Bowen, 1965; Vines, 1964). Scattered evidence can be found to support expectations that each of the other variables in this category will also be related, weakly, to the justices' voting in either civil liberties, economics, or both.[3]

The second category in Table 2 lists the justices' career characteristics. Ulmer's research (1973b) suggests that federal administrative experience should be negatively related to a justice's liberalism on civil liberties issues; Johnston's study (1976) indicates that having been either a federal judge or prosecutor should be negatively related to a justice's liberalism on both civil liberties and economic issues.[4] Lindstrom's work (1968) showed prior judicial experience to be negatively related to a justice's liberalism on civil liberties questions. However, Vines (1964) showed that having held federal elective or judicial office is positively related to southern district judges' decisions on desegregation issues. Most research focusing on non-Supreme Court judges suggests that there are few important relationships between judges' career traits and voting (see Goldman, 1975; Nagel, 1974; Bowen, 1965).

The third category in Table 2 consists of measures of the justices' age and tenure. *Appointment* and *End Tenure Years,* essential for calculating the other age and tenure variables, were also considered in the analyses in order to detect secular trends in Supreme Court justice voting.

A number of researchers have found relation-

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[1] The problem is essentially one of equivalence of measures across the "cultures" represented by differing judicial careers. Students of comparative politics recognize that there are no easy solutions to such problems. But they also recognize that difficulties in establishing equivalence of measurement do not justify abandoning comparative research. (See Holt and Turner, 1970; Przeworski and Teune, 1970; Warwick and Osherson, 1973.)

[2] It proved impossible to code reliably a more direct measure of family status than the education status index. The index is very similar to that used by Bowen (1965, p. 13) to measure family status.

[3] See Bowen (1965, p. 199) for the impact of region and status and Nagel (1974, pp. 266-67) for the impact of prestige of legal education and size of town and Vines (1964) for the impact of southern origins on civil liberties voting.

[4] Exploratory analyses of these data revealed the apparent nature of the statistical interaction which Johnston's "judge of prosecutor" measure may have been tapping. Justices who had been prosecutors were less liberal than non-prosecutor judges, but prosecutor-justices with judicial experience were substantially more liberal than prosecutor-justices without such experience, though still less liberal than non-prosecutor justices. Since this interaction effect can be meaningfully interpreted in terms of the moderating influence of judicial service on the basic civil liberties conservatism induced by prosecutorial experience, it is represented in the *Type of Prosecutorial Experience* index.

Table 2. Supreme Court Justices' Personal Attributes Investigated with Their Operational Definitions

Category 1: Birth, Upbringing, Education

1. Year of birth: last 3 digits coded.
2. Region of birth: 1 = South or border state; 0 = other.
3. Religion: 1 = Non-Protestant; 0 = Protestant.
4. Size of town of upbringing: 1 = urban area; 0 = other.
5. Educational prestige measures
    a. Prestige of prelaw education: 1 = high; 0 = average.
    b. Prestige of legal education: 1 = high; 0 = average.
    c. Education status index: sum of 5a and 5b; range 0-2.

Category 2: Career Characteristics

6. Judicial experience measures
    a. Any judicial experience? 1 = some; 0 = none.
    b. Extensive judicial experience (5 or more years)? 1 = Yes; 0 = No.
    c. Extent of judicial experience index: 2 = 5 or more years; 1 = some, less than 5 years; 0 = none.
    d. Some judicial experience? 1 = some, but less than 5 years; 0 = none or more than 5 years.
7. Compound experience measures
    a. Served as either federal judge or federal prosecutor? 1 = yes; 0 = no.
    b. Type of prosecutorial experience index: 2 = none; 1 = prosecutors with judicial experience; 0 = prosecutors with no judicial experience.
8. Ever hold elective office? 1 = yes; 0 = no.
9. Ever serve in Department of Justice? 1 = yes; 0 = no.
10. Office held at appointment
    a. Federal administrative office (cabinet, etc.)? 1 = yes; 0 = no.
    b. Judicial office? 1 = yes; 0 = no.
    c. Elective office? 1 = yes; 0 = no.
    d. No office held? 1 = yes; 0 = no.
11. Appointment region: 1 = South or border state; 0 = other.

Category 3: Age and Tenure

12. Age at appointment: in years.
13. Appointment year: last 3 digits coded.
14. Year tenure ended (1978 for those still serving): last 3 digits.
15. Tenure on Supreme Court: in years.
16. Age at mid-tenure: in years.
17. Age cohort: 1 = birth post-1900; 0 = birth pre-1901.

Category 4: Partisanship

18. Justice's party identification: 1 = Democrat; 0 = non-Democrat.
19. Appointing president's party identification: 1 = Democrat; 0 = Republican.
20. Party identification indexes:
    a. Party index 1: 3 = justice and appointing president Democrat; 2 = justice Democrat, appointing president Republican; 1 = justice non-Democrat, appointing president Democrat; 0 = justice non-Democrat, appointing president Republican.
    b. Party index 2: 5 = justice and appointing president Democrat; 4 = justice Democrat, appointing president Republican; 3 = justice Independent, appointing president Democrat; 2 = justice Independent, appointing president Republican; 1 = justice Republican, appointing president Democrat; 0 = justice and appointing president Republican.

Continued on next page

Table 2. (continued)

Category 4: Partisanship (continued)

21. Appointing president:
    - a. Roosevelt: 1 = yes; 0 = no.
    - b. Truman: 1 = yes; 0 = no.
    - c. Eisenhower: 1 = yes; 0 = no.
    - d. Kennedy: 1 = yes; 0 = no.
    - e. Johnson: 1 = yes; 0 = no.
    - f. Nixon: 1 = yes; 0 = no.
    - g. Ford: 1 = yes; 0 = no.

Source: Operational definitions devised by the author.

ships between age (variously operationalized) and judicial voting, particularly concerning civil rights and liberties (see Ulmer, 1973b; Lindstrom, 1968; Goldman, 1975). However, others (Nagel, 1974; Bowen, 1965) have found age to be of little value in predicting how judges vote. On balance, the evidence appears to suggest that one should expect to find a relationship between a measure of age and Supreme Court voting behavior, especially in civil rights and liberties. The same is *not* true for tenure: none of the studies cited here demonstrated any substantial or consistent impact of length of service on judicial voting in any type of decision.[5]

The final category in Table 2 is "Partisanship." Many studies have found judges' party identifications to be among the most important influences on their voting across all kinds of issues. (See Johnston, 1976; Lindstrom, 1968; Goldman, 1975; Nagel, 1974; and Bowen, 1965; plus many of the "pioneering studies": Schubert, 1959; Nagel, 1961; Ulmer, 1962; and Goldman, 1966.) Although Ulmer's study (1973b) did not find the justice's party identification to be an important predictor of criminal case voting, it is anticipated here that it will be a significant predictor of Supreme Court justice voting in civil rights and liberties and economics cases.

The correlation (r = .67) between the party identifications of the justices and those of their *Appointing Presidents* makes it problematic that both variables will make independent contributions to the explanation of the justices' voting (but see Lindstrom, 1968, pp. 70-71). The "Party Index" attempts to measure the joint effects of the two identifications.

Because appointing presidents are often very self-conscious about the need to appoint justices who have the correct ideology or "real politics" (Scigliano, 1971), the significance of this variable for shaping Supreme Court decision making would seem obvious. Surprisingly, however, none of the backgrounds-decision studies cited above makes an effort to include the identity of the appointing president as a predictor of judicial voting. This exclusion does not seem justified. The potential significance of *Appointing President* is suggested by some of the conclusions of a recent massive study of the influence of background characteristics on the voting behavior of federal district judges:

> This study presents updated information to reaffirm our belief that the appointing President is a variable of considerable importance in influencing the behavior of trial judges. For example, the percentage of Johnson appointees voting on the liberal side of judicial questions is some 21 points higher than for Nixon appointees for the same years. Our investigation further suggests that the variables of political party affiliation and appointing President are not entirely synonymous. That is, some Presidents have obviously selected (or been able to select) district court judges with greater ideological care than others. . . . Thus . . . to a fair degree, Presidents tend to get out of their judicial appointees the kind of decisions they want from them (Rowland and Carp, 1978, pp. 27-28).

It is anticipated that the appointing president will be a significant influence on Supreme Court voting behavior in civil liberties and economics cases.

To include all the background variables listed in Table 2 in prediction equations for the civil liberties and economics voting behavior of the 25 justices would not have been possible, even if it were wise. The number of variables would have exceeded available degrees of freedom. Furthermore, several of the variables were quite substantially correlated, either because they represented merely alternative ways of coding the same basic characteristic, or because they reflect true "real world" covariances. To include such variables would have led to intolerable multicollinearity in the prediction equations. Beyond such limiting technical considerations, however, was the necessity for parsimony. Good personal attribute explanations of Supreme Court voting behavior should rest upon as few, and as potent, interpretable independent varibles as possible.

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[5] But see Lamb (1976) for some suggestive, though ultimately inconclusive, results on increasing conservatism with aging among court of appeals judges, and Ulmer (1973a, 1979), who shows that a justice's degree of liberalism may vary somewhat across the terms of his tenure. In fact, Ulmer suggests that such variation may even follow distinct curvilinear patterns for some long-serving justices. Of course, the fact that a variable has a variance does not prohibit meaningful analyses based on its central tendency, and it is that sort of analysis which is the focus of this article.

Statistical Procedure. Stepwise regression analysis, including both forward inclusion and backward elimination procedures, was used to assist in determining which attributes were sufficiently potent and stable to merit reporting in the final regression models of "Civil Liberties Liberalism" (%LIBCL) and "Economic Liberalism" (%LIBECON). However, the final composition of these models was in no way the choice of the computer's stepwise algorithms. On the basis of previous research, I assumed that the final models would contain measures of partisanship, particularly justice's party identification and appointing president, religion, some kinds of career experience indicators, and, perhaps, age. I did not automatically accept models emerging from the stepwise selection process which failed to include measures of these attributes. Rather, I estimated the statistical properties of alternative models including these attributes but excluding previously included attributes which were collinear and thus competing with these attributes. In addition, I computed models including alternative measurements of those attributes which *had* alternative measurements and compared them for their statistical potency and stability.

A variable found important in previous research was excluded from the final regression equations only when all model-building and testing procedures suggested that it had no explanatory power or that its explanatory power was a spurious result of its correlation with other, more potent and stable attributes. These final equations, presented in Table 3, included only predictors which were substantively appealing, statistically robust, and minimally collinear; which were highly statistically significant (probability of *F* for the regression less than .001); which contained only predictors with t values whose absolute magnitude exceed 1.25; and which yielded the maximum adjusted R^2.

Table 3. Personal Attribute Models of Liberalism in Voting in Civil Rights and Liberties and Economics Cases for U.S. Supreme Court Justices, 1946-1978

Liberalism on Civil Rights and Liberties Cases (% LIBCL)				Liberalism on Economics Cases (% LIBECON)			
Independent Variable	**Beta**	**B**	**t**	**Independent Variable**	**Beta**	**B**	**t**
Party Identification	.48	29.1	4.58	Party Identification	.73	38.4	4.78
Appointing President:				Appointing President:			
Truman	−.54	−43.9	−5.65	Truman	−.26	−18.5	−1.92
Johnson	.14	14.9	1.45	Nixon	−.19	−13.4	−1.34
Nixon	−.24	−19.2	−2.27	Prestige of Prelaw			
Appointment Region	−.34	−22.2	−3.14	Education	.19	10.0	1.37
Extent of Judicial				Appointed from			
Experience	.17	6.3	1.89	Elective Office	.34	27.0	2.20
Type of Prosecutorial				Extent of Judicial			
Experience	.56	24.2	5.95	Experience	.31	9.9	2.33
				Type of Prosecutorial			
				Experience	.29	10.7	1.97
Intercept		12.0				4.3	
R^2		.87				.72	
Adjusted R^2		.82				.61	
Standard Error of Estimate		13.0				16.5	
F (d.f.)		16.12 (7,17)				6.27 (7,17)	
Probability of F		<.00005				<.00005	
Determinant of Correlation Matrix of Predictors		.45				.44	

Source: Computed from data accompanying John Paul Ryan and C. Neal Tate, *The Supreme Court in American Politics: Policy Through Law,* 2nd ed., Washington, D.C., American Political Science Association, 1980.

Findings

Each attribute model contains seven independent variables. Five variables, *Party Identification, Extent of Judicial Experience, Type of Prosecutorial Experience, Truman Appointee,* and *Nixon Appointee,* are included in both models, thus reflecting the substantial zero-order correlation of these two forms of judicial liberalism. Both models are statistically robust and predictively successful. The model for %LIBCL explains 87 percent of its variance; that for %LIBECON 72 percent of its variance.

Neither model's explanatory power rests to any appreciable degree upon the relatively limited residual degrees of freedom remaining after the seven variables are introduced into the regression equations: the Adjusted R^2 for %LIBCL is .82; for %LIBECON, it is .61. Both models are highly statistically significant, since the probability of their F coefficients is less than one in 20,000. Finally, neither equation is plagued by serious multicollinearity.

Given the inevitable crudity of measurement of the independent variables and the prevailing scholarly understanding that such attributes must be limited in their ability to account for variations in judicial decision making, the levels of statistical explanation achieved by the models must be regarded as impressive.

Civil Rights and Liberties Liberalism. The model depicts the justices' voting on civil liberties cases as a product of their career experiences and partisan affiliations. Its coefficients predict the most liberal score for the fictitious justice who is an experienced judge without prosecutorial experience, and who is a Democrat appointed from a nonsouthern state by Lyndon Johnson. Conversely, its most conservative score is predicted for the also fictitious justice who was a prosecutor without judicial experience, and who is a Republican (or Independent) appointed from the South by Harry Truman.

The %LIBCL model contains seven independent variables, but actually represents only five distinct attributes, since two of the independent variables are dichotomies measuring *Appointing President.* The most significant of these variables, relatively speaking, are *Party Identification, Truman Appointee,* and *Type of Prosecutorial Experience.* It is worth noting that the %LIBCL model does *not* include religion as an independent variable. This finding is surprising, since in the literature Protestantism was consistently associated with conservatism on civil liberties, and since religion had the highest zero-order correlation with %LIBCL. However, extensive analysis of these data has made it clear that, once the impacts of the variables listed in Table 3 have been taken into account, religious affiliation adds little or nothing to the explicability of %LIBCL.

The attributes included in the model for %LIBCL all have sound and interpretable relationships with civil liberties liberalism. Party identification is shown to affect the behavior of America's top judicial elite in the same way it affects that of other political elites and the masses. The influence of the appointing presidents' presumed concern with the values of their appointees is clearly documented. Judges appointed from the southern states are shown to be less favorable toward civil liberties claims than their northern counterparts. And finally, the important interactive efforts of two types of career experience—judicial and prosecutorial—are documented: nonprosecutors are shown to be much more favorable toward civil liberties claims than their prosecutor colleagues, as one would predict, while, among prosecutors, those with some judicial experience are likewise more favorable than those without it, reflecting the moderating influence of sitting on the other side of the bench. Although the most important impact of judicial experience occurs in the just-mentioned moderating effect it has on the behavior of prosecutors, its influence is not limited to that group of justices. The regression coefficient for *Extent of Judicial Experience* demonstrates that the longer a justice's prior service as a judge, the greater his willingness to support civil rights and liberties claims, regardless of his other characteristics.

Economic Liberalism. The overall relationship between civil liberties and economic liberalism is fairly strong ($r = .797$). Thus one should expect considerable congruence between the regression models for these two kinds of behavior. The %LIBECON model in Table 3 shows that congruence. But it also is sufficiently different from the %LIBCL model to make a separate attribute analysis of economic liberalism worthwhile.

The economic liberalism model predicts the most liberal score for the mythical justice who is a Democrat appointed from elective office, but with extensive judicial and no prosecutorial experience, who attended a high-prestige prelaw school and was not appointed by Harry Truman or Richard Nixon. The most conservative score, conversely, is predicted for the again mythical Republican (or Independent) justice who attended an average-prestige undergraduate school, was not appointed from an elective office, had prosecutorial, but not judicial, experience, and was appointed by Harry Truman.

The substantive interpretations of the relationships in the economic liberalism model are generally clear and meaningful. The most consistent dimension related to American partisanship since the 1928-36 realigning election period has been that measured by the economic liberalism index used here. It is thus highly appropriate and consistent with previous research to find that the Supreme Court justices' party identifications are the most important influences on their voting behavior on economic issues. The relationship for appointment from elective office is also meaningful. Mass electorates are more liberal on economic questions than elites. Thus lawyers who have found favor with a mass electorate are likely to be more liberal than their counterparts who continue to labor mostly in the service of the socioeconomic elites. The relationship between %LIBECON and *Prestige of Prelaw Education,* which at first glance may appear to go in an unexpected direction, may also be comfortably interpreted as reflecting the economic conservatism of the self-made man who lacked the initial advantage of a high-prestige education.

The other relationships in the economic liberalism model reflect, with differing magnitudes, relationships in the %LIBCL model. The influence of appointing presidents is again revealed. So are the impacts of judicial and prosecutorial experience. The interpretations of the latter are not as clear-cut in the case of economic liberalism as they were in the case of civil liberties liberalism. But it is clear that judicial experience is a liberalizing influence in the Supreme Court justices' voting on economic issues. It seems likely that judges are more removed from service to the interests of the economically privileged than most non-judge lawyers, and that the longer their service on the bench, the more they find the protection of the interests of economic underdogs to be a central part of their judicial duties. Prosecutorial experience is, on the other hand, a conservatizing influence. Perhaps the effort to convict defendants, who are overwhelmingly economic underdogs, leads prosecutors to have little sympathy for the claims of economic underdogs outside criminal cases. More generally, prosecutors spend most of their time defending the position of the "haves" against the criminal attacks of the "have nots." Such experience would logically engender sympathetic attitudes toward economic "top dogs."

Discussion

The success of the personal attribute models presented here is quite impressive. Statistically, there can be little doubt that they overcome the supposedly inherent limitations on the explanatory potential of background models discussed by the authors cited earlier. If an attribute model can, contrary to prediction, produce strong rather than weak or moderate associations with judicial voting behavior, is one justified in rejecting the argument that attributes are only indirect causes of judicial behavior? I think not. The models reported here refer only to one relatively unique court, and only to the 25 justices serving on that court in the period since the end of World War II. It is not clear that similarly successful models can be constructed for the judges of lower fededral courts or state appeals courts. Thus the findings reported above must be regarded as suggestive, but hardly conclusive, on the utility of attribute models in explaining the decision making of judges in general. Despite this caveat, it is worth exploring why the attribute models presented here achieve very positive results.

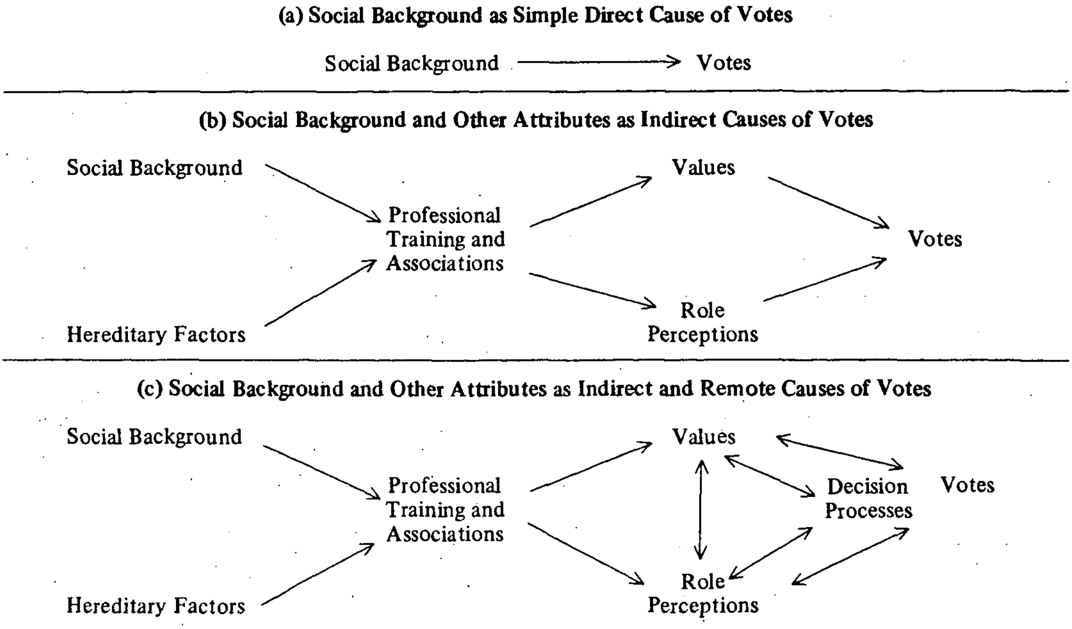
Leaving aside all theoretical considerations, it is clear that these models are predictively successful because of the data and measurement procedures used. This research paid more attention to identifying and including significant measurable attributes relating to the justices' birth and socialization, career, age and tenure, and partisanship than have many previous studies. Equally important, it paid close attention to the operationalization of indicators for the attributes included. Multiple indicators of important attributes were constructed and tested for their predictive efficacy and statistical stability; indicators reflecting potential nonlinear or non-additive effects were constructed and tested for their contribution. Because previous attribute models have often been based on the most readily available data and have used crude and sometimes unimaginative operationalizations of potentially crucial variables and only straightforward linear and additive regression models, it is not surprising that their results should have been less impressive than those reported here.

Skeptical readers may doubt that better measurement and model-building alone could produce these results. In addition, the theoretically inclined reader may assert that attribute models still fly in the face of good theory concerning judicial decision making, no matter how statistically potent they may be. In answer to both, I would argue that at least two other factors may underlie the explanatory success of the attribute models. First, attribute models may not (or need not) be as far removed from properly specified models of judicial decision making as has usually been assumed in the literature. Second, judges' attitudes such as socio-political values or role orientations, which have been assumed to be crucial intervening variables between their attributes and decisions, may be such proximate causes of judicial decisions that they are, for many analytical purposes, all but indistinguishable from decisional behavior. If this should be so, then attribute models could provide highly statistically satisfactory explanations of judicial behavior even though they failed to include the unmeasured (and often unmeasurable) attitudinal variables which intervene between the attributes and decisions in the models.

Figure 1 details three models of judicial voting which Murphy and Tanenhaus (1972, p. 109) put forward to illustrate why social background variables are likely to be only weakly related to decisional behavior such as voting. Figure 1a, which represents a simplistic, straightforward link between social backgrounds and votes, is the model which, Murphy and Tanenhaus contend (1972, p. 108), underlies studies of the impact of social backgrounds on judicial voting behavior. Such a model "so oversimplifies the real world as to be of slight use." Reality, they hypothesize, might better be represented by more intricate models such as those in Figures 1b and 1c.

Certainly the models in Figures 1b and 1c represent richer and more theoretically appealing explanations of judicial decision making than that in Figure 1a. However, it is a mistake to conclude that attribute models must reflect the simplistic logic of Figure 1a. The models tested in this research included not only measures of what Murphy and Tanenhaus appear to mean by "social background" (e.g., size of town of upbringing, educational prestige), but also indicators of "hereditary factors" (birth region, age) and a broad range of measures of the justices' "professional training and associations" (judicial and prosecutorial experience, holding of elective office, etc.). Thus the models in Table 3 are not so far removed from "realistic" models of judicial voting as might have been assumed.

The key difference between the attribute models studied here and the most elaborately specified model in Figure 1c is the former's exclusion of the two categories of attitudinal variables, values and role perceptions, and the decision processes variable category. The exclusion of decision processes is entirely proper for the dependent variables studied here. Decision processes may well intervene between a judges' attitudes, values, and role perceptions and his decision to case a particular vote or even a few votes in a direction not indicated by these characteristics. However, over the hundreds or even thousands of votes cast by the Supreme Court justices included in this study, the influence of decisional processes should be minor and self-canceling. If decisional processes caused a justice to cast a conservative vote on one case when other factors would have produced a liberal vote, they would probably cause him to cast a liberal vote when other factors would have produced a conservative vote on another case. The result would be to remove the influence of


(a) Social Background as Simple Direct Cause of Votes

(b) Social Background and Other Attributes as Indirect Causes of Votes

(c) Social Background and Other Attributes as Indirect and Remote Causes of Votes

Source: Slightly adapted from Walter F. Murphy and Joseph Tanenhaus, *The Study of Public Law*, New York, Random House, 1972, p. 109.

Figure 1. Potential Models of Judges' Voting Behavior

decision processes from models of the career voting behavior of the Supreme Court justices.

The failure of the attribute models considered to include measures of the justices' values and role perceptions is potentially more serious from a theoretical standpoint. There is good evidence to suggest that the voting behavior of Supreme Court justices reflects their values, perhaps fairly closely (see Schubert, 1974; Spaeth, 1979, for example). And while the evidence for the impact of role perceptions on decisions is more mixed (see Vines, 1969; Howard, 1977), recent research (Gibson, 1978) indicates that the interaction of values and perceptions of roles may be very important in explaining at least some kinds of decisional behavior for at least some judges.

Unfortunately, there is no practical way of including measures of the Supreme Court justices' values and role perceptions in this analysis. The first problem is operationalization of these attitudinal measures. Studies of the values of a substantial number of Supreme Court justices have invariably operationalized values from behavioral indicators, usually votes. While criticism has been leveled at the inferential leap involved in defining values from votes, there is no reason to object to this inference as long as the inferred values are not then used to "explain" the judicial voting used to document their existence. Since it is my avowed objective to explain differences in justices' career voting behavior, the usual method of inferring values from votes cannot be used. Some independent measures of values would be required.

It is difficult to imagine what independent measure of judicial values could be used in this study. Attitude surveys, often used to measure the values (and role perceptions) of sitting lower court judges, have their limitations when most of the justices being analyzed are dead. An alternative method, inferring values from written works of judges (Danelski, 1966), might be applicable in principle, but would require a research investment which is well beyond the resources of most researchers if applied to 25 justices.

The independent operationalization of role perceptions poses no more tractable a problem. Dead justices cannot respond to role inventories even if living ones should prove willing to do so, and the content analysis of written materials in search of role perceptions would be just as impractical as for values.

Given the impossibility or the extreme difficulty of operationalizing values and role perceptions for the Supreme Court justices analyzed here, to argue that attribute models are unsatisfactory unless they include such variables is to reject the possibility of useful research on the explanation of Supreme Court justice voting behavior. It does more to establish and protect intellectual sacred cows than to advance our knowledge of judicial behavior. However, before abandoning this discussion of values and role perceptions, one should note once again that the models reported in Table 3 do include at least one attribute—*Appointing President*—which may more or less directly index the values and role perceptions held by Supreme Court justices. The evidence suggests (see Abraham, 1974; Scigliano, 1971) that presidents pay close attention to the values and role perceptions which their potential appointees are likely to bring to the bench. Scigliano (1971, pp. 125-48) concludes that they are much more often than not successful in appointing justices who will display presidentially desired values or role orientations on the Supreme Court. It seems likely, therefore, that the presence and importance of the appointing president measures in the models of Table 3 reflects the values and role perceptions of justices selected by self-conscious presidents. If this can be accepted, the attribute models studied here end up being very like the more theoretically appealing model depicted in Figure 1b, and far removed from the straightforward model of Figure 1a.

The preceding paragraphs have demonstrated that attribute models of judicial decision making need not be so simplistic or atheoretical as has often been assumed. Their explanatory potency may reflect their de facto theoretical appropriateness. There is another possible explanation for the statistical potency of the attribute models. It may be that attitudes reflecting values and role perceptions, the intervening variables ostensibly excluded from attribute models, are so closely linked in a causal sense to voting that one can serve as an operational indicator of the other. The discussion has already noted that judicial values have often been inferred from judicial voting, while the evidence indicates that role perceptions and values are ordinarily substantially correlated (see Howard, 1977; Gibson, 1978). If votes and attitudes are so closely linked that one may be inferred from the other, it would appear that for most analytical purposes they may be indistinguishable. A model which purported to explain votes would be just as good in explaining values. And since attributes are the proximate causes of values and role perceptions in Figure 1b, one might expect attribute models of values/votes to be quite successful predictively.[6]

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[6] Analolgous situations arise in the literature on mass voting behavior. For example, a recent attempt to develop a nonrecursive causal model of American presidential voting choice actually presented models of the causes of voter evaluations of the candidates, since the latter was such a powerful proximate cause of voting choice as to make separate analyses of vote superfluous

The substantial explanatory powers attached to the models in this study may reflect a close causal linkage between judicial values and judicial votes. If values could be independently measured and included in voting models, they might account for virtually all the variance in judicial voting, with the influence of attributes being almost totally channeled through values. Since they cannot be independently measured for inclusion in the models, the influence of attributes is transmitted directly and powerfully to judicial voting behavior.

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Political Decision Making in Face-to-Face Groups: Theory, Methods, and an Empirical Application in Switzerland

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This article presents a model of decision making and introduces a new theoretical variable to the literature, namely, the modes of decision making. The theoretical focus is on the face-to-face group, and the article also develops an innovative methodology for studying this type of decision-making behavior. Variation in the decision modes is explained as a function of the strategic considerations of individual decision makers. These considerations are affected by a set of four independent variables: structure of the decision group, substance of the conflict, context of the conflict, and the decision process. The data, drawn from observations of decision-making groups in Switzerland, are tested with discriminant analysis and a simulation. In both cases total correct classifications exceed 55 percent, indicating that there is a meaningful structure relating variation in the decision modes to the theoretical framework.

How the political scientist should study decision-making behavior remains a perplexing and unresolved issue. The discipline has witnessed the rise (and fall?) of numerous competing approaches, ranging from the exclusively macro-level systems approach and its now much-maligned "black box" to the spate of economic models focusing on the individual, rational decision maker acting largely in isolation. Until fairly recently, political scientists have appeared somewhat wary of studying decision making at a level of analysis that falls somewhere in between these macro- and micro-analytic approaches, namely, at the level of the "face-to-face" group. In other disciplines, notably social psychology, a great deal of research on face-to-face groups has been conducted. Kirkpatrick (1976) points out the utility to political scientists of such a theoretical focus in a special issue of the *American Behavioral Scientist* which focuses on small group theory, while Golembiewski (1970) contains a collection of essays on precisely this topic. Other examples of useful research in this area include Fiorina and Plott (1978), Hinckley (1979), Sullivan (1976), and Verba (1961).

The purpose of this article is to explore the possibility of developing a general theory of political decision making in face-to-face groups. In an earlier study (1980b), our theoretical concern was focused largely at the macro-level of whole political systems: we attempted to demonstrate that what was understood conceptually as "consociational democracy" was in reality a much more complex set of decision-making norms and behavior than heretofore assumed by the theory. In the present context we want to demonstrate the extent to which our theoretical framework can contribute to a broader understanding of a general class of political decision-making behavior in committees.

Data: Background and Description

Our data are drawn from observations of the Free Democratic party of the Berne Canton in Switzerland. The basic research method was participant observation. As is so often the case with research of this nature, access to the decision-making groups was largely the fortuitous result of the social scientist happening to be in the right place at the right time. As a member of the FDP, Jürg Steiner was able to secure the much-needed access not only to all the relevant committee meetings, but also to interview data, recorded minutes of meetings, and to other internal and external flows of communication in the party. During the 21 months in which he carried out his observations (January 1969-September 1970), Steiner observed the decision-making behavior in 111 committee meetings. These meetings ranged in size from the very large party conventions (N = 150) to the very small bureau of the executive committee (four members). Consequently, we are not dealing solely with small face-to-face decision-making

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groups, but with large, even cumbersome ones as well.

Our units of analysis are individual, openly articulated conflicts in the groups. At least two participants had to express explicit disagreement on a particular point before the disagreement could qualify as a conflict. If a conflict reappeared in a later meeting of the same committee, it was treated as a second case because under these new circumstances the actual decision-making process might very well differ from that of the first meeting. In all, we obtained data on 466 such conflicts.

Dependent Variable: Modes of Decision Making

One of the central problems confronting studies of face-to-face decision-making groups is the selection of an appropriate dependent variable. Most studies focus on the decision outcome itself, attempting to explain the group decision as a function of individual preference orderings. Predicting such outcomes generally requires that the researcher have a fairly extensive knowledge of individual preferences prior to and during the decision process. In real-world settings it is often extremely difficult, if not impossible, to obtain such information; recurrent interviewing of participants can seriously disrupt the normal course of political business in any decision-making group.

In this study we will introduce a dependent variable that has received relatively little systematic attention in the professional literature, namely, the modes of decision making. Rather than focusing our attention on the substantive outcome of a decision, we will look instead at the decision mechanism by which that outcome was achieved. The assumption is that the decision outcome will often be strongly influenced by the decision mode. Thus the innovativeness of group decisions may depend on the mechanisms by which groups arrive at their decisions. In addition, the decision modes may have implications for a wide range of variables that are important from the perspective of normative democratic theory, such as the willingness of group members to participate in further meetings and the level of hostility and antagonism in such groups.

Typology

We have developed our typology of decision modes along three dimensions: (1) Whether or not the substance of the conflict was resolved, (2) the degree of formalization of the decision, and (3) the persistence of dissent within the group. When a conflict arises and disagreement is openly articulated, the first question one needs to ask is whether or not the substance of that conflict is resolved in that particular meeting. Although "deciding not to decide" is obviously a decision of sorts, it is limited to the procedural matter of leaving the conflict unresolved. Since the substance of the conflict remains unresolved, the possibility exists that it will be brought up again later; it is also possible that postponement is tantamount to "killing" a proposal, much as occurs at times in the U.S. Congress when a bill is referred back to a committee. Regardless of the specific reasons for postponement or the eventual outcome of a subsequent conflict concerning the issue, we consider these unresolved conflicts as *no decisions*. Obviously, this term has a much narrower definition than the term *nondecision* originally used by Peter Bachrach and Morton S. Baratz (1962).

If the substance of a conflict is resolved by the group, it may be accomplished in a formalized or informalized manner. The possible advantages of an informal decision mechanism include the fact that it reduces the clear distinction between winners and losers, which can at times help to preserve harmonious intragroup relations, and it allows for subtle manipulation of the decision-making process. An informal decision occurs when one (or more) of the participants simply interprets what he/she considers to be the essence of the discussion, and then succeeds in obtaining the tacit support of the other participants for this interpretation. We have labeled this mode of decision making *decision by interpretation* and we can distinguish three ways in which such interpretations are made: (1) by the committee chairperson in a final summary, (2) by the secretary in the minutes, and (3) by individuals in the decision-making process who tacitly interpret the preference of the group and then direct the discussion in such a way that a decision is implicitly made. One of the central characteristics of a *decision by interpretation* is that, while it does resolve the substance of a conflict, it does not give any clear indication of whether the disagreement in the group persists or vanishes in the course of discussion. This decision mode also provides substantial leeway for those group leaders who are adept manipulators of the decision-making process.

A formal decision mode is one in which the participants who have expressed an opinion in the course of a discussion are required to take a visible stand on the issue when a decision is made. If dissent among group members persists, we will have a *majority decision:* a vote will be taken that will distinguish a winning side from a losing side. The criteria for winning may be a simple plurality, an absolute majority or a qualified majority such as two-thirds of the votes. If, however, the disagreement disappears at the end of the discussion, the resulting decision will be achieved through *amicable agreement*. Such an agreement may be

Table 1. Typology of Decision Modes About Manifest Conflicts in Face-to-Face Groups

	Resolved Conflict		Unresolved Conflict
	Formalized Decision	No Formalized Decision	No Decision
Persistence of Dissent	Majority Decision	Decision by Interpretation	
Disappearance of Dissent	Amicable Agreement		

Source: Compiled by the authors.

given through an oral statement or a gesture such as a nodding of the head. The agreement may also be expressed through a unanimous vote. Obviously, amicable agreement means only that the manifest disagreement has disappeared; whether or not latent conflicts persist among the group members is not considered.

Of these four modes of decision making, three should be quite familiar from everyday usage and various treatments in the professional literature. Of course, different labels have been applied to the mechanisms but *majority rule, no decision,* and *amicable agreement* are not fundamentally new concepts. *Decision by interpretation,* however, is a concept that we developed while working within this theoretical context. As we have shown elsewhere (1980a), we feel that such decisions occur frequently in a variety of contexts.

Theoretical Framework

Having sketched the important characteristics of the four modes of decision making, we move now to a consideration of our theoretical framework. Obviously, our task is to explain under what conditions specific decision modes will be employed in face-to-face decision-making groups. Before we can select the independent variables that we will use in explaining this dependent behavior, however, we must specify the theoretical framework from which we will subsequently derive our explanatory variables. We begin with a set of axiomatic assumptions concerning the general behavior of politicians in face-to-face groups and build our theoretical framework deductively, ultimately arriving at a point from which we can select a set of relevant independent variables and a number of hypotheses relating these variables to variation in the modes of decision making.

In general, we assume that politicians are individual value-maximizers. Moreover, given a set of preferences or a rank-ordering of values, these individuals will seek to maximize those values that they consider to be in their best interests. In this basic sense we share the microanalytic, rational-actor focus of most economic decision-making models. However, our theoretical approach departs significantly from these economic models in our conceptualization of the range of values that politicians maximize and the degree to which these values are assumed to remain constant over time and across issues. Specifically, we assume that the range of values can be accurately identified and subsumed under four general headings: *power, group solidarity, rectitude,* and *time.* In addition, we assume that the individual weights assigned to these values will vary from actor to actor and, for each actor, from situation to situation.[1] This assumption introduces a dynamic component to our theory since we assume that no single value will always be the primary goal for every politician in every decision-making situation. The research task is then to specify under what conditions certain values are likely to be more important than others, and hence how that particular value structure is likely to affect the selection of a decision mode.[2]

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[1] The validity of this assumption is supported in the works of Lehner and Schütte (1976) and MacCrimmon and Messick (1976), for example.

We have also assumed that the selection of a decision mode by a face-to-face decision-making group depends to a significant degree on the values that the members of the group try to maximize. So, for example, one might think of an individual politician who would like to see a most preferred outcome reflected in the group decision. If this politician is concerned only with winning a particular conflict (maximizing power), and if he/she believes the necessary majority support exists, he/she might press for a vote. If, on the other hand, one fears that such a vote might lead to divisive intra-group strife, one might try to "interpret" the decision; this would allow one to achieve the objective of "winning" the conflict while at the same time facilitating the preservation of group solidarity by partially concealing the identities of those members who are in the losing minority. A decision maker who feels that the necessary support for a preferred outcome is lacking might pursue a strategy of *no decision* in the hope that postponement would provide sufficient time to muster that additional support. If the individual preferences are relatively weak and concerns with group solidarity are strong, it seems likely that *amicable agreement* will be the desired strategy. If participants are tired and/or frustrated after a lengthy decision process, calling for a quick vote may be desirable if the primary goal has become finishing the business at hand rather than finding an optimal decision outcome. In other words, the selection of a particular decision mode is assumed to be the aggregate result of the strategic considerations of individual participants in the group decision-making process. These strategic considerations, moreover, will reflect the interplay of the four values included in our model.

Of course, we anticipate that most decision-making situations will be characterized not by the dominance of a single value but by an interplay of the four. In many cases different participants will be motivated by different values in the same decision situation. Consequently, even our relatively short list of four values presents us with a potentially staggering number of logically possible combinations. When we add the further theoretical consideration that the rank-ordering of the four values can change for each individual within each decision situation, it becomes apparent that formulating hypotheses for all of the logically possible combinations would be a nearly endless and perhaps fruitless endeavor. Rather than pursuing this particular avenue in the development of our theory, we try instead to decide on the basis of our independent variables which values are likely to be most important for the largest number of participants (with some additional consideration of weighting for the more powerful group members). From this information we then make probabilistic estimates of the likelihoods of occurrence for the four modes of decision making. Although we will therefore be unable to test empirically each of the posited relationships between the values and the decision modes at the micro-level, we will be able to determine whether our more aggregate predictions based on these microanalytic linkages correspond to the observed outcomes. In the end, the fit between our model's predictions and the observed outcomes should provide some measure of the overall plausibility of the micro-level theoretical reasoning that led to the predictions. The next question, then, is: what variables influence the values that political decision makers maximize and hence, the modes of decision making that a face-to-face group employs?

Independent Variables and Hypotheses

Previous research and our knowledge of the relevant literature have helped us arrive at four sets of potential explanatory variables. These sets are organized under the following headings:

1. Structure of the decision group
2. Substance of the conflict
3. Context of the conflict
4. Decision process

It seems wholly plausible that the values that an individual decision maker chooses to maximize will depend on the attributes of the group within which the decision process takes place. Equally plausible is the notion that these values will be affected by the kind of conflict being debated. We also expect the broader context within which a conflict takes place to have an impact on these values. Finally, since we have stressed that we expect these values to change under certain conditions once the discussion unfolds, we assume that these values will be affected by the structure of the decision process.

A wide range of variables was employed for the four sets (see Table 2). For each of the variables we formulated a detailed hypothesis relating change in the variable to changes in the values and ultimately to changes in the modes of decision making. Each of these hypotheses was formulated prior to the completion of any data analysis; hence the nature of the hypothesized predictions is entirely a priori. Rather than presenting each of the independent variables and the related hypotheses, we shall illustrate our general procedure by way of a single example.

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[2] A description and analysis of these values may be found in Dorff (1978).

For our example we have chosen the variable, "size of the group." First, we concentrate on the way in which the four values may be influenced by the size of the decision group. Perhaps the most obvious impact can be seen for the opportunity costs of lost time. In a large group there are more potential speakers, which raises the prospect of a lengthy discussion. At the same time the typical group member has relatively few opportunities to intervene in the discussion, and much time will be spent simply listening to other members. This mostly passive role, particularly when extended over a lengthy meeting, may contribute increasingly to the feeling that time could be better invested for other purposes. Thus the incentive grows to reduce the opportunity costs represented by this loss of valuable time. In a small group, on the other hand, the discussion tends to be shorter and everyone can participate more fully. Consequently, the decision makers generally do not perceive the time spent in the decision process as a significant opportunity cost.

In addition to its effect on considerations of time, the size of a group probably influences the importance of group solidarity as an incentive as well. Solidarity is expected to be more important in small groups because the low number of participants will lead to a high number of interpersonal interactions and, according to George Homans' well-known hypothesis, frequent interactions should be a favorable condition for solidarity (Homans, 1961). Another argument, based on Olson's work, states that in a small group each actor can contribute in a more perceptible way to maintaining the group, whereas maintaining a large group depends much less clearly on the actions and inactions of individual group members (1965, p. 65). Thus in a small group the motivation of a rational actor to work for group solidarity should be much greater than in a large group.

We anticipate that considerations of power will have a greater impact in large groups than in small groups. In part, this is due to the effect of considerations of solidarity as noted in the preceding paragraph. In addition, the larger the number of people attending a meeting, the more that meeting takes on the character of a spectacle; the more the participants feel as though they are "performing" in this spectacle, the more power considerations will be involved. Winning or losing has a much more dramatic effect in a large group than in a small group, and scoring a point in the closeness and informality of a small group is much less spectacular than scoring the same point in the limelight of a large meeting.

We can find no logical reasons why the size of the group would have an influence on considerations of rectitude.

The causal linkage between the size of the group, the values, and the modes of decision making can now be made. The pressure for solidarity in a small group can be best met with a decision by

Table 2. Independent Variables

Structure of Decision Group

1. Size
2. Predominance of low-status participants
3. Frequency of interactions among the members
   - –Frequency of meetings
   - –Number of governmental functions of the members
4. Age of the members
   - –Biological age
   - –Political age
5. Homogeneity
   - –By language
   - –By sex
   - –By religion
   - –By education
   - –By occupation
   - –By rural background
   - –By place of residence*

Substance of Conflict

6. Valence nature
7. Reference group
8. Innovativeness
9. Personnel versus policy conflict
10. Perceived importance of conflict
    - –Participation rate at the meeting
    - –Length of the discussion

Context of Conflict

11. Time distance to parliamentary elections
12. Unlikelihood of referendum
13. Pre-parliamentary versus parliamentary stage
14. Party control of government

Decision Process

15. Formality of discussion
16. Focus of discussion
    - –Number of agenda points in meeting
    - –Number of issues within the agenda point under discussion
    - –Number of decisions in meeting
17. Timing of the discussion
18. Certainty of information
19. Number of proposals
20. Origin of proposals
21. Structure of debate
    - –Distribution of opinions
    - –Number of speakers with clearly expressed opinion

Source: Compiled by the authors.

*The directional sign for this variable is reversed from the other homogeneity variables. In this case a high positive value indicates a heterogeneous group; a high negative value indicates a homogeneous group.

amicable agreement. Two potential obstacles to this decision mode are not particularly salient in small groups, namely, time pressure and considerations of power. If amicable agreement cannot be achieved in a small group, the second best choice seems to be *no decision* or *decision by interpretation*, both of which would not disrupt solidarity significantly. A *majority decision* appears to be the least desirable choice since it may be detrimental to solidarity. In large groups, an important consideration is the time factor which can be best served with a *majority decision*. A *majority decision* also contributes to the concerns with power since it allows each participant to stand visibly by an original position. Of course, standing by one's position can result in a loss of power if that position winds up in the minority. However, since group solidarity is relatively unimportant in large groups, the majority will be largely unconcerned with what happens to the minority, so concessions will be unlikely. *Amicable agreement* should be quite infrequent in large groups since achieving such agreement is both time-consuming and premised on the salience of group solidarity.

After formulating all of our hypotheses in this manner, we proceeded to the empirical analyses.

Statistical Methods of Analysis

As a response to the categorical nature of our dependent variable, we first employed discriminant analysis. The technical nature and theoretical underpinnings of this technique have been discussed elsewhere (Dorff, 1978). For the moment we shall point out that this technique is designed to find a particular linear combination of independent variables that will maximally discriminate between two or more groups of observations.[3] That combination (or combinations) is then used to classify the individual observations in one of the categories of the dependent variable. The more clearly the categories are distinguished by the p-dimensional vector(s) comprised of the p independent variables, the more successful or accurate will be the classification procedure. The vectors provide some indication of the relative strengths (contributions) of the independent variables in distinguishing among the categories, while the classification scheme indicates the extent to which the combination(s) accurately differentiate(s) among the groups. It is in this very general sense that comparisons are often drawn between the vector of b's and the R^2 in regression analysis and the discriminant functions and the successful classification rate in discriminant analysis.

A second, major component of our analysis was a simulation in which we attempted to classify each conflict according to the decision mode most likely to be employed under the conditions specified by the independent variables. Rather than reducing the scope and precision of our predictions by choosing more than one possible outcome, we have made single-point predictions; in other words, if two decision modes are considered likely to occur but one seems more likely than the other, we have chosen the most likely decision mode as our single prediction. It should be clear, however, that this single prediction does not reflect the fact that our theory postulates a non-zero probability for the occurrence of other decision modes (although not in all cases). Making this single prediction will mean that our overall success rate wil be considerably lower than if we made more than one prediction.

Empirical Tests

Discriminant Analysis. In Tables 3 and 4 we have reproduced the results of the discriminant analysis. Three components of these results deserve special treatment: (1) the discriminant functions and the variable loadings, (2) the location of the group centroids in the reduced space, and (3) the classification table.

1. *Discriminant Functions.* Discriminant functions are similar to factors in a factor analysis in that the loadings of the individual variables on the function provide some indication of the relative contribution that an independent variable makes to the discrimination among the groups; simply stated, higher loadings on the discriminant function imply greater significance of those variables. The ten variables contained in Table 3 are drawn from a pool of 21 independent variables originally employed in the discriminant analysis (see Table 2).[4]

2. *Centroids of Groups in Reduced Space.* These figures indicate the locations of the four group means in the reduced three-dimensional space defined by the three discriminant functions.

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[3] Technically, this amounts to solving the basic characteristic equation problem:

$$(W^{-1}B - \lambda I)d = 0 \text{ when } W \neq 0.$$

[4] This reduced list of independent variables was derived from the original 21 variables through the use of a preliminary set of bivariate analyses. The criteria for selection or exclusion included both the apparent strength of the relationship and the degree to which the nature of the relationship corresponded to our original hypotheses. For a detailed discussion of each of these variables and the bivariate analyses, see Dorff (1978), Chs. 5-8.

Table 3. Discriminant Analysis Results

Variable	Standardized Discriminant Function Coefficients: Function 1	Function 2	Function 3
Structure of Decision Group			
1. Size	.08	.09	−.48*
2. Predominance of low-status participants	−.55*	.40*	.48*
3. Frequency of interactions among members			
–Frequency of meetings	.02	−.39*	−.13
–Number of governmental functions of the members	−.66*	.54*	.49*
4. Homogeneity			
–By rural background	−.09	−.11	.42*
–By place of residence**	−.40*	.07	.11
Substance of Conflict			
5. Valence nature of conflict	.58*	.03	.19
Context of Conflict			
6. Time distance to parliamentary elections	−.10	−.32	.53*
7. Unlikelihood of referendum	−.05	.34*	−.46*
Decision Process			
8. Formality of discussion	.26	−.49*	−.18
9. Focus of discussion			
–Number of agenda points in meeting	.20	−.24	.43*
10. Number of proposals	.09	−.19	−.36*

	Centroids of Groups in Reduced Space: Function 1	Function 2	Function 3
Majority Decision (MD)	−.33	−1.31	−.12
Amicable Agreement (AA)	.54	−.04	.60
No Decision (ND)	−.63	.28	.14
Decision by Interpretation (DI)	.29	.18	−.41

Source: Developed by the authors from data in Robert H. Dorff, "Employing Simulation Analysis in Decision-Making Theory," Ph.D. Dissertation, University of North Carolina, Chapel Hill, 1978.

*Important variables are at least half as large as the absolute value of the largest coefficient.

**The directional sign for this variable is reversed from the other homogeneity variables. In this case a high positive value indicates a heterogeneous group; a high negative value indicates a homogeneous group.

The greater the relative distance between the means, the more the groups are differentiated along that dimension. When we combine the directional sign (+ or —) for the group mean with the directional sign for the independent variables on the discriminant function, we have an indication not only of the strength or contribution of the variables but also of the direction of the impact on the likelihood of each of the four decision modes. In order to assess this impact, we must examine the three functions in order to determine which groups are being most clearly discriminated.

A glance at the centroids for function 1 reveals that it discriminates most clearly between ND (—.63) and AA (.54), with a secondary discrimination between MD (—.33) and DI (.29). Table 3 reveals further that an increase in the predominance of low-status actors pushes the discriminant function in a negative direction. Consequently, as more low-status participants are involved, the function takes on an increasingly negative value and ND and MD become much more likely to occur while AA and DI become less likely. In the initial formulation of our hypotheses we had anticipated just such a result: low-status participants, being less concerned with group solidarity (AA) and too inexperienced to be able to manipulate the decision-making process (DI), are likely to vote often and to postpone decisions. When we look at the discriminant function as a whole and try to characterize it according to the major variables contributing to it, we find that the function takes on its highest positive value (thus favoring AA and DI) under the following conditions: (1) *group structure* characterized by a predominance of high-status actors who have shared a place of residence (Berne) and who have not accumulated numerous governmental functions, and (2) the *substance* of the conflict characterized as a valence issue. The finding concerning the number of governmental functions runs counter to our expectations. We hypothesized that as the number of these functions increased, the likelihood of interaction among group members outside of this group's meetings would increase. In so doing, this would lead to greater concerns with solidarity. The other findings are in accordance with our expectations.

The second discriminant function distinguishes almost exclusively between MD (—1.31) and the three other modes of decision making. In this case five variables make the most significant contribution to the discriminant function. The relative explanatory power of the second and third functions is less than the first since, much as in a factor analysis, they pick up only that information not contained in the first function. When we look once again at the entire function and attempt to characterize it according to the five variables, we find that the function takes on its highest negative value (i.e., favoring MD) when: (1) *group structure* is characterized by frequent meetings, a predominance of high status actors, and a low group average for governmental functions, (2) the *context* is one in which a referendum is certain, and (3) the *decision process* is formal. Under these conditions, after having "controlled" for the information contained in the first discriminant function, the results indicate that MD would be the most likely outcome.

The primary contribution of the third discriminant function is the discrimination between AA (.60) and DI (—.41), a feature that completes the discrimination process by distinguishing between the two decision modes that previously remained relatively close together in the multidimensional space defined by the independent variables. The fact that eight variables make significant contributions to this function is a further indication of the decreasing importance of the functions; in other words, the function is expectedly more diffuse. Nevertheless, we can still attempt to characterize this function generally according to these variables. Such an attempt reveals that the function would take on its highest positive value when the following conditions exist: (1) *group structure* characterized as a small group, dominated by low status actors, primarily homogeneous with regard to rural residence, and with a high average number of governmental functions, (2) a *context* consisting of impending or recently held elections and a high possibility of a referendum on the issue, and (3) a *decision process* that is relatively unfocused (many agenda points) with only a few proposals on the isue. In such situations it is likely that AA would be employed and unlikely that DI would result. With the notable exception ofthe status variable, these results bear a close correspondence to the hypotheses that we formulated prior to the analysis.

3. *Classification Table.* This is the most important component of the discriminant analysis. In Table 4 we have reproduced the results of the classification procedure based on the preceding discriminant functions. For each observed decision an overall score on each of the discriminant functions is calculated. The three scores for each observation are then compared in order to predict the value of the dependent variable on the basis of the observed values of the independent variables. The classification table then indicates both the predicted and actual outcome for the observations, as well as an overall measure of percent correctly classified. The elements on the main diagonal of Table 4 are the numbers and percentages of cases correctly classified for each of the four groups.

In the present context we shall focus only on the total number of cases correctly classified rather than on the individual groups. Because of missing values for 30 of our 466 decisions, 436 cases were available for classification. Of these 242 or 55.5 percent were correctly classified according to the single mode of decision making that was employed. The question now concerns the establishment of a suitable baseline or chance model against which to compare this result in order to determine how well our model fared. In other words, what rate of successful classification might we expect if we had no theoretical model, and how does our model then perform in terms of predictive capability?

The baseline that we have chosen is derived from LaPlace's Rule of Insufficient Reason, whereby one simply guesses each category the same number of times as every other category. Our primary reason for selecting this baseline is the fact that it incorporates no prior knowledge of the marginal distribution of the dependent variable, as do other possible baselines.[5] Since prior probabilities were set equal (i.e., .25) in the discriminant analysis, these predictions were not based on prior knowledge of the actual distribution of the decision modes. Similarly, the predictions in our simulation are a priori and not based on the marginal distribution of the four decision modes. Consequently, the expected proportion of correct predictions that we employ as our baseline is 25 percent.[6]

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[5] For example, we might guess each category only as often as its observed frequency, leading to a baseline figure of 28.4 percent [(.124) (.124) + (.206) (.206) + (.305) (.305) + (.365) (.365)]. Alternatively, we could simply guess the modal category (decision by interpretation) each time and achieve a 36.5 percent successful classification rate. The use of such information concerning the marginal distribution of the dependent variable seems inappropriate for evaluating predictions made a priori, as suggested by Hildebrand et al. (1976).

Having selected an appropriate baseline figure, we must now devise some means of evaluating our model's performance as compared to the chance model. Such a procedure can be derived from the general model for defining a proportionate reduction in error measure (PRE) (Hildebrand et al., 1976, 1977). By modifying the PRE measures slightly, we calculate the difference between the actual percentage of correct predictions obtained from the analysis and the number of correct predictions that one would expect from chance alone, and then divide this difference by the number of correct predictions expected from chance.[7] This calculation then provides us with a measure of the proportional increase in accuracy attributable to our model.

Taking the 55.5 percent successful classification rate, and employing 25 percent as the appropriate baseline, our proportional increase in accuracy measure is calculated:

$$\frac{55.5 - 25}{25} = \frac{30.5}{25} = 1.22.$$

According to this measure, we are able to improve our classification by 122 percent over what we would expect from chance alone.[8] This increase represents a substantial improvement in our ability to classify the original observations according to the mode of decision making actually employed.

A closer examination of the important variables in the three discriminant functions reveals that group structure is a key determinant of the difference among decision modes; in all three functions variables reflecting characteristics of group structure are the most numerous. But the substance and context of the conflict, as well as the decision process, also contribute to the variation in likelihood of the decision modes. This conforms to our general model of decision making discussed earlier: group structure is the first variable that enters our flow process, and hence may be considered the most important simply because it sets the stage for the actual decision making that follows; this structure is then mediated by the type of issue being considered, the context within which the conflict takes place, and the ensuing decision process. Although discriminant analysis cannot deal ade-

Table 4. Classification Results

Actual Outcome	Total No. of Cases	Predicted Outcome Majority Decision	 Amicable Agreement	 No Decision	 Decision by Interpretation
Majority Decision	43	30 *69.8%*	4 9.3%	4 9.3%	5 11.6%
Amicable Agreement	93	12 12.9%	47 *50.5%*	16 17.2%	18 19.4%
No Decision	134	16 11.9%	17 12.7%	76 *56.7%*	25 18.7%
Decision by Interpretation	166	22 13.3%	28 16.9%	27 16.3%	89 *53.6%*

Correctly Classified = 55.5%

Source: Developed by the authors from data in Robert H. Dorff, "Employing Simulation Analysis in Decision-Making Theory," Ph.D. Dissertation, University of North Carolina, Chapel Hill, 1978.

Note: The italicized percentages on the main diagonal represent the percent of cases correctly classified into each of the four categories.

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[6] (.25) (.124) + (.25) (.206) + (.25) (.305) + (.25) (.365) = .25 or 25 percent.

[7] A similar and perhaps more familiar measure is based on Goodman's and Kruskal's Lambda and is discussed in Weisberg (1978, p. 562). See also Hildebrand et al. (1976, pp. 519-21).

[8] In addition to classifying the entire data set we also performed a split half-sample analysis. This standard cross-validation procedure, conducted for both the pooled and unpooled versions of the discriminant analysis, indicated that the overall classification results are perhaps somewhat overstated as a consequence of being maximized to this specific data set. However, since the pooled version results varied only between 42 percent and 63 percent, our estimate of 55.5 percent seems to be quite reasonable. For a discussion of this cross-validation procedure see Dorff (1978, pp. 139-41).

quately with the flow of this process, it does appear to highlight the relative temporal importance of each set of variables. In the next step we shall try to incorporate this flow process more systematically in our analysis through the use of simulation.

Simulation Analysis. In addition to discriminant analysis, we also constructed a simulation for analyzing our data. The purpose of this simulation was to create, a priori, a classification procedure that would predict the decision mode on the basis of a reduced set of independent variables drawn from our original pool of variables. The primary strength of simulation analysis lies in its ability to handle the "flow process" nature of our model. In other words, the dynamic branching effects that we believe most accurately characterize this type of decision-making process will be replicated within the context of our simulation. Once again, we can evaluate the model's performance on the basis of the total number of cases correctly classified. We remind the reader of our earlier discussion of the single-point nature of these predictions and the implications for assessing the overall accuracy of the predictive model.

On the basis of the discriminant analysis and general correspondence with our original theoretical propositions we selected seven independent variables for our simulation.[9] The variables and the sets from which they are drawn are as follows:

I. Group Structure
  1. Size of the Group
  2. Political Status of the Group Members
II. Substance of the Conflict
  3. Position versus Valence Issues
  4. Reference Group of the Conflict
III. Context of the Conflict
  5. Time before Parliamentary Elections
  6. Likelihood of a Referendum on the Issue
IV. Decision Process
  7. Origin and Number of the Proposals

For the sake of clarity we have briefly stated our hypotheses in Table 5.

The first variable that enters the process is *group structure*. From the moment a decision group convenes, it takes on relatively fixed structural characteristics. In particular, each group will be either small, medium, or large and will consist solely or predominantly of high-status or low-status actors. From our theory and the individual hypotheses contained in Table 5 (Hypotheses 1 and 2), we would expect that group structure alone would not suffice for the prediction of a single decision mode. Our theory presumes that certain tendencies should be evident under specific structural conditions but that an actual outcome will remain largely indeterminate until further information becomes available. In other words, while certain structural combinations will clearly favor one or two decision modes over the others, group structure alone should rarely (if ever) be sufficient for determining a single decision mode. Therefore, our simulation would make note of the specific characteristics of the group structure but would pass directly on to the next variable (substance) rather than taking the path to the predictions.

Once the meeting is convened, the group is confronted by an agenda of issues. In each decision-making situation, then, the actual *substance of the conflict* begins to interact with the characteristics of the group structure. In our model we distinguish between conflicts dealing with actual goals (position) and conflicts dealing with means (valence). Further, we consider the group to which a conflict refers an important determinant of the way in which the decision-making process unfolds and, ultimately, of the decision mode actually employed. However, we also expect that the criterion of position-valence will be somewhat more important than the reference group, although the interaction between the two remains crucial. Our assumption follows from the argument that the position-valence nature of a conflict will be the primary characteristic of the substance; political decision makers will consider this factor first, and only secondarily will they consider the reference group to which a particular issue refers.

In general, we also assume that the combined information regarding structure and substance will not suffice for specific predictions. Our reasoning here is that the group structure and substance of a conflict, while of primary importance in determining the resulting decision mode, will not always lead in the same direction; hence some indeterminacy must remain. Moreover, most combinations of these two sets of variables will serve only to narrow the range of possible decision modes (for example, from four to three to two) rather than to restrict that range to a single mode which we could then predict. The exception to this indeterminacy occurs for small groups comprised solely of high-status actors or solely of low-status actors.

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[9] Once again the basis for reducing the number of independent variables consists of a combination of the results of empirical tests (bivariate and discriminant analyses) and the degree to which the suggested relationship corresponded logically to our original hypotheses. If, for example, a variable displayed some discriminating power but the direction was difficult to justify in terms of our theoretical framework, we chose to exclude that variable from the simulation. The selection of these variables is discussed in detail in Dorff (1978, pp. 285-95).

The *context of the conflict* is the third factor to affect the flow of the decision-making process. The two variables that we employ are the time interval to the parliamentary elections and the likelihood of a referendum. One might argue that the time before the elections should enter the process earlier than we have stated; however, the logic of our theory indicates that concern with elections will become salient only if the particular issue being considered is somehow relevant to the upcoming elections. Concerns with electoral impact should normally become salient only after the substance of a conflict is apparent and the preliminary discussion has taken place.

The final variable concerns the *decision process*. Once a meeting is convened, an issue determined, and the impact of contextual variables ascertained, the actual way in which the discussion proceeds will help determine which values will change and which will remain fixed. As we have pointed out earlier, we do not assume that the values for every actor will remain invariant; in

Flow Chart of Decision-Making Process

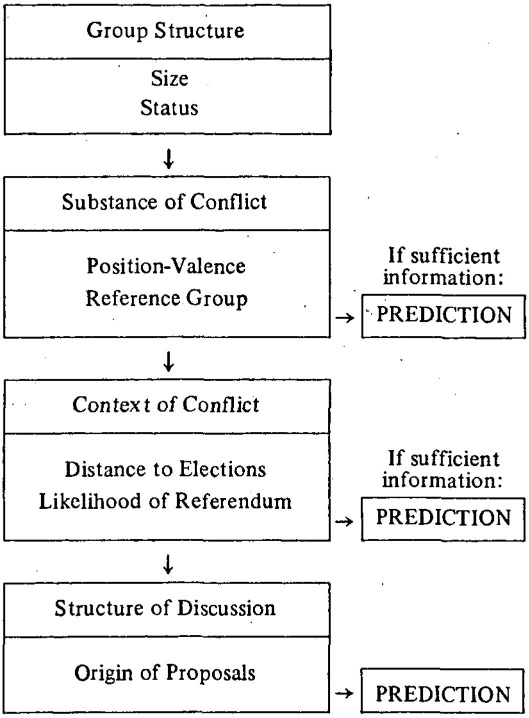

Table 5. Hypotheses for Independent Variables in Simulation

	... the more the probability of ...			
Independent Variables	majority decision	amicable agreement	no decision	decision by interpretation
1. The smaller the *size* of the decision group ...	decreases	increases	is unaffected	is unaffected
2. The larger the portion of high-*status* actors in a decision group ...	decreases	increases	is unaffected	is unaffected
3. The more a conflict deals not only with means but also with *ends* ...	increases	decreases	increases	decreases
4. The more a conflict *refers* to matters of the decision group itself ...	is unaffected	increases	decreases	increases
5. The closer the time distance to the next parliamentary *election* ...	increases	increases	decreases	decreases
6. The greater the likelihood of a *referendum* about the issue under consideration ...	increases	is unaffected	decreases	decreases
7. The more the high-status actors in the group participate in the *articulation* of proposals ...	decreases	increases	is unaffected	increases

Source: Compiled by the authors.

fact, we make the explicit assumption that these values change from actor to actor and, for each actor, from situation to situation. We anticipate that the origin of the proposals (whether it originates with a high- or low-status participant) will be a major factor in effecting change in these values and, hence, in the decision mode that is employed.

Limitations of space simply do not allow us to provide an extensive treatment of our simulation model. Rather, we shall provide a very simple illustration of the model's performance.

For our illustrative example we have chosen one of the simplest cases because it can be described briefly. In this case only three variables are needed before we can make a prediction; the reason for this relative paucity of information lies in the fact that all three variables point to the occurrence of a single decision mode. Therefore, the combined interaction of the three variables should make this single decision mode occur quite often.

The three characteristics associated with this class of decisions are: a small group (seven or less) comprised solely of high-status actors who are discussing a valence issue. A quick glance at Table 5 reveals that our hypotheses anticipate that all three conditions strongly favor *amicable agreement,* while both *no decision* and *decision by interpretation* seem largely unaffected. All three variables indicate that a *majority decision* is highly unlikely under these conditions. Therefore, we make the following predictions: *amicable agreement* is the most likely outcome and is our single-point prediction; *no decision* and *decisions by interpretation* occupy an intermediate position in which they are less likely to occur than *amicable agreement* but not entirely unlikely; *majority decisions* are predicted not to occur.

Our simulation, then, makes the preceding predictions and then compares them with the actual outcomes of decisions made under this particular set of circumstances. As it turns out, ten of the observed decisions were made under these conditions. Of these, six were made by amicable agreement, two by no decision, and two by decision by interpretation. For this set of decisions we have therefore achieved a successful classification rate of 60 percent. Again, however, we stress the fact that this figure does not reflect the accuracy with which we predicted the *non*-occurrence of *majority decisions* nor the seemingly correct predictions concerning the intermediate likelihoods of both *no decision* and *decision by interpretation.* While most of our predictions were considerably more difficult to make than these, and of course some were considerably less accurate, this example should illustrate the nature of the prediction process and how the results were obtained.

The entire classification process covered 436 cases. From our universe of 466 decisions we had to omit 30 cases that had missing values on some of the seven independent variables included in the simulation. For 25 of the remaining 436 decisions our prediction was *indeterminate,* leaving us with 411 cases for which we have actually made a single-outcome prediction. In 237 cases (57.7 percent) our predictions were correct. Employing our previous standard of evaluation we find that this figure represents a substantial improvement in our ability to classify each decision according to the actual mode of decision making that was employed. If we adopt a baseline of 25 percent, which presumes that we have no a priori knowledge of the distribution of the four modes, our 57.7 percent correct classification rate constitutes a 131 percent proportional increase in accuracy in our ability to predict the modes of decision making.

Conclusion

The results of the analysis indicate that our theoretical framework makes a significant contribution to an understanding of these group decision-making processes. The relative accuracy and compatibility of the classification results demonstrate that a substantial amount of the variation in the modes of decision making can be explained by the independent variables included in our model. But what contribution does this study make to a more general theory of decision making in face-to-face groups?

First, an examination of our independent variables would show that almost all of them are of a highly general nature. With the exception of the referendum variable, all could be applied to committee decision-making processes in a wide variety of national contexts, and the hypotheses we formulated were not simply case-specific.

Second, our dependent variable is equally broad in its range of potential applications. During the spring and summer of 1979, Robert Dorff conducted a series of 12 decision-making experiments employing graduate and undergraduate students. The groups consisted of either five or ten students. Preferences were induced monetarily and participants received a modicum of instructions—they were told only that they had to choose one of five decision outcomes on each of an indefinite series of decisions. No instructions were given concerning the decision mechanism. Yet in these experimental groups we observed a significant use of *majority decision, amicable agreement,* and even *decision by interpretation; no decision* was of course not allowed. Moreover, the experimental control for group size also resulted in clear support for our hypothesis: the larger groups voted much more often on precisely the same items as small groups, which were much more inclined to employ amicable agreement. These experiments clearly indicate that a theoretical focus on the modes of decision making has a broad applicability and could be useful in any political context where decisions are made in face-to-face groups. The precise refinement of our theory to deal with this more general class of behavior is the goal of our current research efforts.

Third, the contribution of our theoretical framework is not limited solely to its general applicability but includes the fact that it provides a more tractable and meaningful way of studying real-world committee decision-making behavior. Because we were able to gather almost all of our data through participant observation without grossly disturbing the decision-making groups, our methodology should open the door to more frequent and in-depth analysis of such groups. In this way, perhaps, some of the gap between the study of experimental and real-world groups might be narrowed.

In further work we have also examined some of the consequences of the modes of decision making. In particular, we have studied the effects of the decision modes on (1) the willingness of political actors to participate in meetings, (2) the level of antagonism and hatred in the group, and (3) the potential innovativeness of group decisions. Finally, since so much political decision making takes place in face-to-face groups, it is important to see what kind of group decision-making behavior we consider more or less democratic. After all, process as well as outcome is important to an understanding of political decision making. A focus on the modes of decision making should allow us to deal with these issues as well.

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Congressional Staff Turnover and the Ties-That-Bind

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Individual members of the U.S. Congress work with staffs ranging in size from a minimum of 18 to a maximum of well over 100. Each member has come to preside over a personnel system and, consequently, may best be understood as an enterprise *manager. In this article we examine these congressional enterprises in order to gain some insight about the autonomy of individual staffers, the stability of these member-centered enterprises, and generally, the responsiveness of staffers to member objectives. While it is difficult to observe or measure responsiveness directly, some purchase on the question is gained through an investigation of the causes and consequences of staff turnover; this is the major empirical focus of the research reported here.*

Government authority, however much constrained by law or custom, is exercised by people. Analysis of that exercise must concern itself with who those people are, how they came to positions of power and influence, and how long they last. In this article we focus on the rates and patterns of turnover among congressional staff. We do so in an effort to assess the consequences for power and accountability in the legislative process resulting from the enormous growth in size of congressional staff (Fox and Hammond, 1977; Malbin, 1976, 1980; Machowsky, 1978; Brady, 1979; Commission on Administrative Review, 1977). Our basic concern is to establish whether staffers are autonomous political actors and, if so, to what extent they operate outside the control of their nominal superiors, or, alternatively, are bound reasonably closely to the elected members. Turnover is not the only indicator by which to gain insight on this issue, of course, but it is highly useful, both for the immediate purpose and more generally as one way to characterize a political order.

At one level, the comings and goings of public officials are indistinguishable from petty gossip. On other occasions, as Michels, Mosca and Pareto have pointed out, they can comprise the foundation of a wide-ranging theoretical argument. The circulation of elites—whether by election or appointment—has often been taken as a hallmark of responsiveness to shifting majorities or changing priorities. The price for this sort of responsiveness, however, is often a reduction in experienced-based expertise, a consequent decline in effectiveness, and the unhappy prospect of neophytes left in charge of complex and vital matters. A Jeffersonian revolution every 20 years may well be a purifying nostrum for the body politic, but it may also debilitate the effective prosecution of government affairs.

Turnover, then, is a generic property of personnel systems, broadly construed, with systemic responsiveness and performance often serving as both cause and effect. Despite the seriousness and pervasiveness of the matter, however, we have few comprehensive examinations of the phenomenon of turnover, and consequently it is not easy to determine whether any particular magnitude we might observe constitutes much or little.[1] In turn, therefore, we are at something of a loss to know what to make of the numbers. For instance, there have been five presidents between 1960 and 1980. Only one previous 20-year segment of American political experience, the uncertain antebellum years of 1840-1860, witnessed more frequent turnover in that office. Does that matter? Twentieth-century Supreme Court justices have a mean tenure of just over 14 years. Is that high or low in relation to our normative concerns?

Stanley et al. (1967), and more recently Heclo (1977), have calculated turnover rates for executive branch personnel. Cabinet secretaries last some 40 months on average (except for Nixon's appointees who managed only about 18 months), but the usual figure given for under- and assistant secretaries is only 22 months. There is, moreover, great variation from agency to agency. Commerce Department officials have brief tenure, presumably because they tend to hasten back to the busi-

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[1] Among the exceptions to this general assertion are Schlesinger (1966), Hyneman (1938), Prewitt (1970).

ness world. Agriculture and Labor officials would seem to have fewer alternatives; in any event, they remain in office considerably longer. Heclo makes a major point of the fact that the senior political appointees are veritable birds of passage while the top career officials and the "non-career" supergrades have far more longevity and, presumably, more understanding of how Washington games are played.

On the congressional side, much has been written in recent years about turnover, or the lack of it, among members of Congress (Fiorina, 1977a, 1977b; Ferejohn, 1977; Mayhew, 1974; Burnham, 1975). For a time it appeared that incumbent senators were successfully insulating themselves from electoral challenge, though that trend seems now to have reversed (especially in light of the 1980 election). House members continue to enjoy relative security, and the "case of the vanishing marginals" has occasioned both analytic and normative attention.[2] Even while electoral defeat seldom strikes incumbent representatives, however, voluntary retirements of various kinds have left the House with a noticeably less experienced membership than it has had for several decades (Bibby, Mann, and Ornstein, 1980, Tables 1-7 and 3-7; *Congressional Quarterly Weekly Report,* January 12, 1980, 79-82). The consequent weakening of traditional norms and the fading of institutional memory generate severe problems in the legislative process so that, even with high incumbent security, turnover is great enough to undermine what Polsby (1968) and others thought was a thoroughly professionalized institution.

The amount and significance of personnel turnover obviously varies across time and among institutional settings, and it is essential therefore to establish what context provides the relevant setting if the analysis of turnover is to be illuminating. In the congressional case, we would suggest that a focus on the individual members is not completely satisfactory. Contemporary members operate in multiple organizational settings: personal offices, committees and subcommittees, leadership structures, party groups, state delegations, and single-purpose caucuses, to name the more salient examples. A recent organizational survey counted, as of 1978, 990 congressional subunits possessing staff complements either from a direct appropriation or member contributions.[3] Individual members work with staffs ranging in size from a minimum of 18 to a maximum of well over 100. Each member has come to preside over a personnel *system* of greater or lesser complexity and scope. The member of Congress may best be understood, therefore, as an *enterprise,* and the analysis of phenomena like personnel turnover or elite circulation must be analyzed in terms of the dynamics within those member enterprises.[4]

This study will provide an initial charting of this recent organizational development. Our basic premise, as noted, is that the House (Senate) is no longer best conceived of as a body of 435 (100) individuals, or even as a structurally differentiated (committees, subcommittees) collection of legislators, but rather as a collection of organizations or member-centered enterprises. Legislative politics, consequently, consists of relationships both among and within these enterprises, much as contemporary economic activity consists of relationships among (competition, collusion) and decision making within firms (Williamson, 1975). Through a specific focus on turnover within enterprises, our study provides a partial test of hypotheses concerned with enterprise stability and enterprise responsiveness to its nominal head, the member of Congress.

Staff Turnover: Categories of Explanation

Turnover, though normally counted and measured in the aggregate, is the product of a host of individual choices. Discussions are often couched almost entirely in impersonal terms, treating both the members of Congress and the staff as collectivities and considering primarily the systemic consequences of turnover. We must, however, give appropriate recognition to the fact that we are dealing with people, with individuals who are driven by ambition, who respond to incentives, and who run the gamut of personalities and motivations. In this section, we briefly examine three broad categories of explanation of staff turnover: (1) individual motives and ambitions, (2) relationships with members of Congress, and (3) situational features. These categories and interpretations are derived from extended interviews with nearly 100 House and Senate staffers during 1978-1979.

Individual Motives. Individual motivation in its substantive detail is, of course, unique to each case, but some more generic categories of motivation may be discerned and some illustrations may be offered as to how they operate. First of all, there is the matter of ambition. The proportion of those who join congressional staffs but who aspire to eventual careers outside the Hill community is surely rather large. Some are looking for the credentialing experience, others hope to parlay their contacts and experience into private-sector fame and fortune, while a few hope to run for office themselves. Indeed, it is rather rare for young staffers to look upon their present role as offering a lifetime career. Staff roles in Congress and elsewhere in public life are generally too loosely defined, compared—say—to teaching or medicine, to permit outsiders to contemplate spending their working lives there. While some of this ambition may be directed toward advancement within the staff system, the most characteristic form of ambition among staff is external, oriented toward career success outside the staff structure, and this provides an important explanation for high turnover.

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[2] For a different and more somber view of the prospects of incumbents, see Erikson (1976).

[3] The details are found in Salisbury and Shepsle (1979).

[4] For a related point of view with respect to the House of Representatives, see Loomis (1979).

If ambition is a driving force leading to turnover, the tangible benefits of staff employment work in the opposite direction. The pay is good and, for the younger staff especially, it is often far higher than alternative employment would bring. Retirement benefits are excellent, and the superiority of the congressional retirement system quite often attracts executive agency personnel and holds them for extended periods of service. The pace of life for committee staffers is somewhat less frantic than that on personal staffs, and job security, while never assured, has seldom been severely challenged in recent years for most staffers most of the time.

Nevertheless, staff positions of all kinds are inherently frustrating. They preclude, by definition, personally labeled accomplishment. They require, if not entire anonymity, at least a willingness to subsist on glory reflected from the principal. Only a few insiders will know that an eloquent speech, a crucial amendment, or a clever maneuver was actually the hard work of a staffer, not the member of Congress whose name gave it both identity and political significance. Clearly, people differ in their willingness to pursue careers of being "on tap, not on top."[5] The norms of professionalism may mitigate the frustrations of anonymity and hold a staffer in a position for an extended time. So may whatever job security there is, for with security the congressional staffer comes to resemble any other bureaucrat. With the lack of acknowledged accomplishment there is a commensurate lack of responsibility, so that the staffer does not routinely have to make the tough policy decision or implement the risky political strategy. For the temperamentally risk-averse, the staff role may well be rather comfortable.

If we were to conjecture about how individual motivations combine with experiential factors to encourage a staffer to leave or remain on a particular staff, we would anticipate, in the first few years of employment, a fairly high tolerance for the frustration, the heavy and unpredictable workload, the limited rewards (other than pecuniary ones), and relatively unpleasant working conditions. The most common initial motivation for work on the Hill possesses all the characteristics of an investment; it is a stepping stone from which to launch a career elsewhere. The likelihood that a staff member will leave increases with each passing year. There is, therefore, a fairly steady egress of people who have obtained—say—two to four years of credentialing experience. The probability that a staff employee will not remain another year hits a maximum after about four years, and thereafter begins to decline as responsibilities and rewards increase and the employee recognizes the attractions of serving 20 years in order to cash in on a generous retirement (often at a fairly young age). If, in fact, a staffer's turnover likelihood is curvilinear in his or her Hill experience, all other things equal, then an interesting connection between staff turnover and growth is implied. Increases in neophyte staffers, which come with expansion of the staff system, increase the pool of most probable "turnovers" so that growth and turnover go hand in hand, though possibly with some lag. We examine the evidence on this implication below.

Relationships with Members. Much is often made of the distinction between personal and committee staff.[6] Certainly there are differences in rules and regulations pertaining to these different categories. Yet too much is sometimes made of this nominal distinction. The fact that individual staffers are on different payrolls camouflages a central feature of the employment relation that holds for the wide majority of congressional staffers namely, that they are *member-specific resources*. While this is obvious in the case of personal staff, it is less apparent but nonetheless true for most committee and subcommittee staff, too. Staff, whether personal or committee, are hired to serve the policy, electoral, and even ideological objectives of the members who lead the various congressional enterprises—personal offices, committees, subcommittees, etc. (Kampelman, 1954). Occasionally, the authority to employ staff is delegated, as when a member gives an Administrative

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[5] This classic phrase was used to characterize the personnel who were to assist the president in staff roles in the 1937 Brownlow Commission's Report on Administrative Management. See Karl (1963).

[6] The standard reference is Kofmehl (1963). For a lucid overview, see Patterson (1970).

Assistant (AA) responsibility for hiring or a chairperson allows other committee members to nominate committee employees. But in nearly every case, a staff employee is associated with a member-patron. In recent years, new rules in each chamber (S. Res. 60 and H. Res. 5) have made de jure what had been a fairly widespread practice of converting nominally collective staff resources into member-specific resources. These rules gave individual members (in the Senate) and subcommittee chairpersons (in the House) access to committee funds to hire staff who, though nominally employed by the whole committee, are in fact under the instruction and direction of individual members.

Thus, although there are exceptions, the overwhelming majority of individual staffers in every nominal category are tied to a principal or patron. Consequently, while some employment conditions are shared by all staffers, many depend rather on the idiosyncracies of principals. It follows from this that the personal disposition of staffers to stay or leave is heavily dependent on ties that bind them to their principals, the members of Congress. Since the large majority of senior staff are, or feel themselves to be, closely linked to specific members, either because the staff appointments resulted from some quasi-patronage connection or because the intimate working relationship between staffer and member breeds a personal interdependence, such ties may not be readily reestablished when the member in question moves on. To the extent that staff is thus closely tied to the fortunes of individual members, their turnover will likewise follow the movements of members, not only movement out of the Congress but within it from one chamber to another or from one committee or subcommittee to another.

Situational Features. We have suggested that both staffers' ambitions and their attachments to a boss affect the likelihood that their employment relationship will be severed. This decision, however, is bound to involve some assessment of alternative opportunities as well. Obviously, if continued employment is no longer feasible—for example, if the member leaves the Congress or fires the staffer—then there is no decision to be made; turnover must occur. But a good deal of turnover is caused by voluntary, calculated decisions made by staff members. Thus, it is a function of both motives *and* opportunities. That is, ambition and opportunity are complementary. We have already discussed ambition; here we focus on opportunity.

1. *Member turnover:* Member turnover not only provides the occasion for turnover of the staffs of those members who have moved on; it provides new opportunities for other staffers as well. New members of Congress, new committee and subcommittee chairpersons, and new partisan and institutional leaders recruit staff. While some of this recruitment is normally drawn from previous associations, particularly in campaigns,[7] a substantial portion comes from the existing pool of employed staffers. Some of these staffers have lost a boss, as in the case of the AA whose member retires or loses an election and who then shops around for another member to serve.[8] Other staffers are on the lookout to improve their financial position and upgrade their job responsibilities. Member turnover provides these individuals with new employment opportunities.

2. *Staff growth:* While we have sought to distinguish growth and turnover, the former creates pressure for the latter in two distinct ways. First, as noted above, new recruits coming from outside the staff system, often outside the Washington community, are the most likely to depart after a short credentialing stay. Thus, we may expect, with some lag, to observe growth-induced turnover of this type. Second, staff expansion provides expanded opportunities for *existing* staff. Some turnover is thereby directly induced which, in turn, has a lagged "echo" as new staffers are recruited to fill these recently vacated positions—new staffers some of whom, themselves, depart after a brief stay.

3. *Institutional change:* One of the bases of staff growth which, in turn, has turnover effects is change in institutional structure. In particular, both the House and the Senate have witnessed heady doses of structural change in the 1970s—creation and elimination of standing committees, growth in subcommittees, new rules for selecting

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[7] Loomis (1979, p. 48) reports an unpublished study of the new members of the 94th Congress that "showed that over two-thirds (68 percent) of administrative assistants and almost half (47 percent) the legislative assistants had worked in their freshman's campaign."

[8] Perhaps the most amusing illustration of "shopping around" involved the staff of Representative Newton Steers (R-Md.) after his election defeat in 1978. The entire Steers staff advertised in Washington area newspapers, offering themselves as a package to new members of Congress. The advertisement read, in part:

> Super staff seeks legislator interested in innovative constituent service, problem-solving legislation to replace super boss hit from behind in the Nov. 7 election. Staff called excellent by area newspaper. Comes complete with legislative assistants, special projects, case and staff assistants, and beautiful black secretary. The complete set—$288,000.

committee and subcommittee chairpersons and members, administrative decentralization of committee business to subcommittees, etc. Many of these changes constitute situational features that produce opportunities or necessities for staff movement.

4. *Executive branch growth:* One of the ambitions staffers often express is the desire to "go downtown." Those who have been closely associated with legislation may seek positions in an executive agency where they will have the authority to implement and administer programs dear to their hearts. For the more senior staffer, an assistant secretaryship for this or that policy area, head of congressional liaison for a federal agency, or service on a regulatory commission is neither an unusual nor an unreasonable ambition. Even in the absence of growth, therefore, normal executive branch turnover will have repercussions for the congressional personnel system. When the executive branch grows, either adding personnel layers to existing agencies or creating new agencies, congressional staff comprise an obvious recruitment pool. A special case of this exogenous force for turnover occurs with partisan changes in the executive branch.

5. *Change in partisan control of the executive branch:* In the inaugural years of 1961, 1969, and 1977, when partisan control of the presidency changed, there was substantial entry into and departure from the executive branch as new personnel joined the Kennedy, Nixon, and Carter administrations, respectively, while Eisenhower, Johnson, and Nixon-Ford appointees, respectively, sought alternative employment. The effect on Capitol Hill personnel of partisan change in the executive branch is twofold: (1) congressional staff are obvious candidates for administration jobs; and (2) executive branch emigrés find on Capitol Hill an obvious market for their substantive and political skills.

6. *Growth in the Washington community:* The Washington commercial property market is booming with law and accounting firms, trade associations, and other interest groups scrambling for office space. Washington recently surpassed New York as headquarters for more national associations than any other city. The "Washington Community," that admixture of public and private sectors whose elements are concerned with the substance and politics of government, is a growing concern. And with that growth come opportunities. Indeed, for many congressional staffers, employment on the Hill is taken in anticipation of a vice-presidency for governmental affairs with some corporation or association located in the "K Street Corridor" downtown. Also on the rise is the number of specialized law firms and lobbying enterprises largely comprised of former congressional staffers.[9] Thus, a growing Washington community produces yet another set of stimuli leading to congressional staff turnover, providing opportunities for and skimming talent from the legislative staff.

In sum, situational factors combine with staff motives and member-staff relationships to account for staff turnover.[10] In the next section, we put some empirical meat on this explanatory frame.

Empirical Results

In the analysis that follows, we examine the continuity of an employment relationship between a staffer and a member-patron and/or between a staffer and an organizational unit. We trace this relationship over time and, in each instance, endeavor to make clear exactly how we counted or measured. Unless otherwise noted, we rely on the *Congressional Staff Directory* listings which are not without their inconsistencies and occasional lapses.[11] The directories, in turn, depend on the members of Congress and the ways in which they choose to identify and label staff members. The vagaries of titles and definitions of responsibility preclude some of the measurements we would have wanted to make. As a consequence, it is important to underscore not only the normal disclaimer owing to measurement imprecision, but also the fact that these data have not been collected with our questions in mind. Nevertheless, a body of data routinely collected in the same format on an annual basis for nearly two decades is not without its advantages for the questions we wish to pursue.

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[9] See "Practicing Law in Washington—An American Growth Industry," *National Journal,* February 4, 1978, pp. 172-79. A recent article in the *Wall Street Journal* (January 15, 1980 p. 1) reports on a study by Boyden Associates, Inc., which found that 20 percent of the top Washington-based executives of big corporations and trade associations were formerly employed on congressional staffs.

[10] One significant situational factor, partisan change in the control of the House or Senate, has not come into play during the time period of our data. With the 1980 election resulting in Republican control of the Senate, staff turnover will be quite large, apart from the effects caused by member turnover, as old majority committee staffs shrink and old minority committee staffs grow. This development will require closer scrutiny.

[11] Let us here register both a confession and a caveat. We rely throughout our analysis on the annual listings of permanent staff compiled by the *Congressional Staff Directory.* We do this because these listings are available since 1962 and because we hope that however much additional variation there may be, the annual rosters will reflect roughly comparable methods of counting. We are fully aware, however, that month-to-month rosters would reveal more variance, and that the highly flexible lists of investigative staff would reveal further elements of personnel use. Individuals are shifted from one payroll to another according to many different needs, especially during the period of reelection campaigns. We hope and believe that it is still reasonable to treat our questions with the data we herewith present, but it must be recognized that our numbers, like those of other studies we have cited, fall well short of the firm empirical baseline measurements we would prefer.

Motivations and Ambitions. To examine questions of individual motivation and ambition properly requires individual-level data of a type we do not have available. There is, however, one implication of our earlier discussion on which aggregate data can be brought to bear. That is the relationship between turnover and growth. If, as we argued above, turnover, conceptualized as the probability of not serving another year on a given staff, is highest among neophyte staffers for whom Hill employment is a credentialing or an employment opportunity-producing experience, then committee staff growth should be expected to induce turnover. That is, growth in year t should, at years t_1, t_2, and so on, have a turnover "echo." Second, growth in any given year should have a contemporaneous effect on turnover by providing new employment opportunities for current staffers.

The data with which to test the two parts of this hypothesis consist of a committee staff turnover time series for each chamber measured by the percentage of committee staff listed in the *Congressional Staff Directory* for year t who are not listed with the same committee in year t_1 and a committee staff growth time series measured for each year (t_1) as the percentage change from the preceding year (t), the latter as reported by the Clerk of the House and the Secretary of the Senate.[12] Thus for each chamber we have two separate time series reflecting, respectively, the aggregate experience that chamber's committees have had regarding turnover and growth.

The contemporaneous correlation of growth with turnover and "echo" effects are given in Table 1. In addition, because after the Watergate hearings the Senate had some sizable reductions in staff size, the three correlations for the Senate are recalculated with 1976 and 1977 data deleted; these are reported parenthetically. Both the contemporaneous and lagged correlations are significantly different from zero for both chambers and the simple regressions of chamber staff turnover against chamber growth, in either the contemporaneous or lagged form, yield significant positive coefficients for the growth variables. Moreover, it would appear that the House and the revised Senate associations are similar in magnitude, with contemporaneous associations substantially larger than the lagged relationships. On the basis of this simple analysis, then, we find support for the hypothesis that growth and turnover are associated. While neither the magnitudes of the regression coefficients nor the amount of variance explained is overwhelming, they are nevertheless statistically significant and positively related as anticipated in our argument. The relationship itself, however, appears to have changed over time. Prior to 1970, committee staff turnover was relatively small and was uncorrelated with committee staff growth (the latter was also relatively small). Since 1970, both turnover and growth have been both large and highly correlated. Statistically, it appears that the relationship between turnover and growth has become quite significant as their respective magnitudes have grown large. Thus, the linear hypothesis (upon which the Pearson r is premised) constitutes a misspecification—the table entries understate the current association and overstate the one in the earlier period. These results, however, are quite compatible with, though a modification of, the qualitative idea that growth alters both current employment opportunities (the contemporaneous effect) and the relative proportions of neophytes and experienced staffers (the lagged effect).[13] Since many other factors affect turnover in addition to growth, we could hardly expect more compelling empirical results.

There is little more we can say at this time about personal motivations and their effect on the turnover decision. There is one other body of data, however, that sheds some light on the ambition hypothesis, viz., that staffers are outward looking, using a few years of Hill employment ultimately to acquire a position off the Hill. For two post-election transitions, 1966-67 and 1976-77, we looked at the personal staff of departing Congress members to determine how many of them left with their bosses and how many remained on the Hill in other capacities. These data are given in Table 2, which gives the proportion of departing members' staffs who were not listed on some other personal or committee staff in either cham-

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[12] The actual time series are not displayed here because of space limitations, but are available from the authors.

[13] Of course, both turnover and growth could be spuriously related through other variables, but fancier statistical models to uncover such effects hardly seem appropriate in light of the crude data we are employing. We acknowledge and thank the anonymous referee of an earlier draft who called our attention to the inconstancy of the relationship over time.

Table 1. Turnover and Growth Hypothesis, 1962-1978

Association of Growth with Turnover	Pearson Correlation House	Pearson Correlation Senate*
Contemporaneous Correlation	.50	.20 (.61)
One-period Lagged Correlation	.33	.24 (.31)
Two-period Lagged Correlation	.26	.70 (.37)

Source: Congressional Staff Directory, various years; *Report of the Clerk of the House,* various years; *Report of the Secretary of the Senate,* various years.

*The correlation between growth and turnover, with post-Watergate years (1976, 1977) deleted, is reported parenthetically.

ber in the following year. There appears to be no temporal effect, but moderate-to-strong chamber and party effect, with Republican and House staffers tending to remain on the Hill more frequently than their counterparts. It is possible, though implausible, that after more than a year's lag, some rejoin Hill staffs; but the bottom line is that, among personal staff, most descend from the Hill on the occasion of their patron's defeat or retirement to seek employment elsewhere. This is spectacularly true of Senate Democratic personal staffers, who leave the Hill at better than an 80 percent rate. For some staffers, it might be argued that these results do not reflect, as we have contended, a *motivation* to seek employment elsewhere after a credentialing experience; rather, they reflect the *unemployability* of these staffers elsewhere on the Hill. Surely this is so for some and, in fact, we have already observed that ambition and opportunity are complements. Yet the magnitudes in Table 2 are so large that we cannot but believe that most of the departures from the Hill on the occasion of defeats and retirements reflect a preference for alternative employment, not the paucity of Hill opportunities.

Table 2. Personal Staff of Departing Members Who Also Leave the Hill (Percent)

	House 1967	House 1977	Senate 1967	Senate 1977
Democrats	72	64	82	82
N =	(295)	(289)	(56)	(207)
Republicans	49	63	64	78
N =	(61)	(147)	(28)	(153)
Total	68	64	76	78
N =	(356)	(436)	(84)	(360)

Source: Congressional Staff Directory, 1966, 1967, 1976, 1977.

Member-Staffer Relationships. In remarking earlier that the personal disposition of staffers to stay or leave is heavily dependent on the ties that bind them to their principals, the members of Congress, we underscored the empirical importance of the relationship between member and staffers. Here we examine several of the empirical manifestations of this ties-that-bind hypothesis.

A crude indicator of the hypothesis in question was given in the previous table. When members leave the House or Senate, not only does their personal staff turn over (by definition), but on average 66 percent of the House staffs and nearly 78 percent of the Senate staffs leave the Hill altogether. Of those that remain, we found it remarkable to discover how many are associated with House members who cross chamber boundaries. In Table 3 we examine the two most recent elections to determine the frequency with which former House members elected to the Senate recruit their new staffs from among their old. These data indicate that the ties that bind appear to be quite strong.

The other side of the coin considers the staff recruits of newly elected members. We examined, for the years 1967 and 1977, the extent to which new representatives and senators select staffers with prior Hill background. While it appears somewhat more common than in the past to recruit experienced personal staff (especially in the Senate), it is further testimony in support of the ties-that-bind hypothesis that most recruitment by new members comes from *off* the Hill, often from campaign staffs and previous acquaintances in the district or state (despite warnings from congressional party leaders to seek out experienced staff assistance)—a point recently confirmed by

Table 3. House Personal Staff Following Representative to the Senate (Percent)

	1977	1979
Democrats	63	79
N =	(28)	(33)
Republicans	43	57
N =	(14)	(56)
Total	58	65
N =	(42)	(89)

Source: Congressional Staff Directory, 1976, 1977, 1978, 1979.

Loomis (1979) for the House. In both 1967 and 1977, new House members, Republicans and Democrats alike, chose only about one-fourth of their staffs from among those with previous Hill experience. In the Senate, the figures were 19 percent and 30 percent, respectively, again with no significant partisan differences.

An examination of the ties-that-bind hypothesis with regard to *committee* staff provides a somewhat different picture. Tables 4 and 5 provide mean staff turnover for the period 1962-78 for each standing committee in the House and Senate, respectively, as drawn from the *Congressional Staff Directory* of each year. We have broken down the computation according to whether the turnover occurred in years in which the identity of the committee chairperson changed, in years bracketing such change (thus allowing for both anticipatory and lagged turnover), and in years with no chairperson change. The test recorded in the first column consists of comparing whether turnover recorded in the second column is less than the maximum of columns (3) and (4). To illustrate, the House Agriculture Committee is consistent with the ties-that-bind hypothesis since turnover in non-chairmanship change years (16 percent of the 287 staffers listed in the *Congressional Staff Directory* in non-chairmanship change years were not listed in the following year) was less than the maximum of turnover in chairmanship change years (24 percent) or "bracketed" years (22 percent)—in fact, it was less than either. Only the Veterans' Affairs Committee in the House and the Agriculture, Armed Services, Foreign Relations, and Interior committees in the Senate fail this test (Senate Judiciary could not be tested since there were no chairmanship changes during the period in question). However, the test is not a very stringent one. For example, comparing (2) and (3) and considering the data to be consistent with the ties-that-bind hypothesis if (3) exceeds (2) by at least five percentage points—a seat-of-the-

Table 4. Ties-That-Bind Hypothesis: House Committee Staff, 1962-78

	(1) Consistency with TTB Hypothesis[b]	Mean Committee Turnover[a]					
		(2) Years with No Chairperson Change		(3) Years with Chairperson Change		(4) Year of, Year before, Year after Chairperson Change	
		%	N	%	N	%	N
Agriculture	Yes	16	(287)	24	(24)	22	(104)
Appropriations	Yes	15	(100)	89	(18)	42	(38)
Armed Services	Yes	11	(296)	13	(60)	9	(185)
Banking and Currency	Yes	43	(298)	35	(20)	58	(105)
Budget	Yes	20	(122)	23	(66)	21	(122)
District of Columbia	Yes	27	(226)	89	(9)	48	(31)
Education and Labor	Yes	17	(446)	56	(27)	27	(64)
Foreign Affairs	Yes	16	(361)	40	(60)	26	(163)
Government Operations	Yes	18	(205)	27	(30)	21	(96)
House Administration	Yes	29	(135)	47	(45)	35	(127)
Interior	Yes	14	(224)	26	(50)	19	(141)
Interstate	Yes	18	(379)	22	(18)	22	(55)
Judiciary	Yes	13	(200)	50	(18)	27	(51)
Merchant Marines	Yes	9	(209)	38	(64)	18	(167)
Post Office	Yes	20	(382)	27	(56)	15	(144)
Public Works	Yes	16	(277)	23	(117)	19	(308)
Rules	Yes	15	(84)	30	(33)	23	(83)
Science and Astronautics	Yes	25	(309)	26	(27)	19	(72)
Small Business	Yes	27	(45)	38	(26)	31	(71)
Standards	Yes	15	(47)	25	(4)	36	(14)
Un-American Activities	Yes	19	(238)	33	(48)	23	(146)
Veterans Affairs	No	16	(218)	10	(31)	12	(82)
Ways and Means	Yes	15	(483)	23	(30)	14	(107)

Source: Congressional Staff Directory, various years.

[a] Each entry gives the mean staff turnover, by committee and leadership change contingency, for the period 1962-78.

[b] TTB Hypothesis: Turnover occurs contemporaneously with, in anticipation of, or in response to committee leadership changes.

Table 5. Ties-That-Bind Hypothesis: Senate Committee Staff, 1962-78

		Mean Committee Turnover[a]					
	(1) Consistency with TTB Hypothesis[b]	(2) Years with No Chairperson Change		(3) Years with Chairperson Change		(4) Year of, Year before, Year after Chairperson Change	
		%	N	%	N	%	N
Aeronautics and Space	Yes	19	(144)	30	(23)	27	(51)
Agriculture	No	17	(173)	0	(7)	0	(22)
Appropriations	Yes	10	(448)	16	(176)	13	(323)
Armed Services	No	14	(249)	11	(9)	11	(28)
Banking and Currency	Yes	18	(280)	37	(41)	27	(111)
Commerce	Yes	29	(631)	26	(68)	41	(129)
District of Columbia	Yes	34	(116)	61	(23)	51	(51)
Finance	Yes	17	(246)	33	(6)	20	(20)
Foreign Relations	No	18	(465)	9	(34)	9	(111)
Governmental Affairs	Yes	16	(180)	67	(24)	37	(62)
Interior	No	27	(355)	9	(11)	21	(28)
Judiciary	NA	14	(181)	–	(0)	–	(0)
Labor and Public Welfare	Yes	33	(368)	40	(50)	28	(120)
Post Office	Yes	28	(186)	54	(28)	36	(81)
Public Works	Yes	20	(306)	42	(12)	23	(40)
Rules	Yes	25	(132)	27	(49)	26	(98)
Veterans Affairs	Yes	30	(87)	81	(16)	38	(50)

Source: Congressional Staff Directory, various years.

[a] Each entry gives the mean staff turnover, by committee and leadership change contingency, for the period 1962-78.

[b] TTB Hypothesis: Turnover occurs contemporaneously with, in anticipation of, or in response to committee leadership changes.

pants difference-of-means test—we find that six committees in the House and six in the Senate fail the test.

More damaging to the ties-that-bind hypothesis, even if turnover in years of chairperson change tends to exceed that in other years, is that despite considerable variation, there is relatively little committee staff turnover even in years of chairperson change. For 13 of the 23 House standing committees and 9 of the 16 Senate standing committees for which there were data, less than one-third of the staff turned over when the committee chairmanship changed hands. While these data hardly force us to embrace the notion of an entrenched bureaucracy, it does appear that committee staff turnover is far less dependent on member-patron relationships than personal staff turnover. Committee staffers tend, with some frequency, to survive chairperson changes (though there is an occasional "big" number in column 3) so that their turnover rates are considerably more independent of member turnover than a strict reading of the ties-that-bind hypothesis would require.

There are some qualifications to this moderately negative reading of committee staff turnover vis-à-vis the ties-that-bind hypothesis. As noted earlier, a number of staff are nominally appointed by the chair, but, in fact, are chosen by others. Thus, some minority staff and/or subcommittee staff are listed in the *Congressional Staff Directory* as full-committee staff (apparently appointed by the chair) when, in fact, they are not. Since they do not leave when a chairperson departs, they are counted as evidence against the ties-that-bind hypothesis when they probably should not be counted at all. A second mitigating circumstance is the capacity of a staffer to serve loyally his or her boss, the chairperson, while at the same time demonstrating to other senior comittee members (and future chairpersons) a capacity for loyalty, ability, and general value to *any* chair of the committee. That such a staffer is retained after a committee leadership change may not constitute a strong violation, and is certainly in the spirit, of the ties-that-bind notion. Here, loyalty, ability, and capacity for good working relationships involve a staffer and several patrons. Thus, while the ties that bind between *committee* staff and chair are probably somewhat stronger than what has been revealed in Tables 4 and 5, they still remain less conspicuous and less constraining than in the case of *personal* staff.

Presumably, even if committee staff turnover were not very high in years of chairperson changes, one might derive some confidence in the superior position of elected officials and the subservient, responsive position of staff so long as staff *leaders* were replaced when committee chairs changed hands. One can see from the data in Tables 6 and 7 that only some confidence is warranted. Sixteen of 22 House committees and 14 of 17 Senate committees pass the weak consistency test described above. But only six of the House committees and seven of the Senate committees *always* experienced staff director changes in conjunction with chairmanship changes. Thus, turnover of neither committee staffs nor committee staff directors is sufficiently frequent or sufficiently related to committee leadership changes to disabuse us of the notion that committee staffs have some of the characteristics of a permanent bureaucracy.

Situational Factors. In this last empirical subsection, we examine a few of the situational factors tht affect staff turnover decisions, though others must await more extensive empirical inquiry. Our focus is chiefly on committee staff.

Consider, first, an institutional change hypothesis. The two houses of Congress have undergone some major changes in the last decade (Ornstein, 1975; Dodd and Oppenheimer, 1977). In the House, beginning in 1971 but especially after the 1974 election, there has been tremendous acceleration in a secular change having its roots in the early twentieth-century revolt against Speaker Cannon—decentralization and the impoverishment of leadership. The major consequences of

Table 6. Ties-That-Bind Hypothesis: Changes in House Staff Directors, 1962-78

	(1) Consistency with TTB Hypothesis[b]	Mean Committee Turnover[a]					
		(2) Years with No Chairperson Change		(3) Years with Chairperson Change		(4) Year of, Year before, Year after Chair-person Change	
		%	N	%	N	%	N
Agriculture	Yes	7	(15)	0	(2)	17	(6)
Appropriations	No	13	(16)	0	(1)	0	(3)
Armed Services	Yes	7	(14)	67	(3)	22	(9)
Banking and Currency	Yes	7	(15)	0	(2)	17	(6)
Budget	Yes	25	(4)	0	(1)	33	(3)
District of Columbia	Yes	19	(16)	100	(1)	33	(3)
Education and Labor	Yes	13	(16)	100	(1)	67	(3)
Government Operations	Yes	7	(15)	50	(2)	33	(6)
House Administration	Yes	21	(14)	33	(3)	33	(9)
Interior	No	13	(15)	0	(2)	0	(6)
International Relations (Foreign Affairs)	Yes	13	(16)	100	(1)	33	(3)
Interstate	No	0	(16)	0	(1)	0	(3)
Judiciary	Yes	6	(16)	100	(1)	33	(3)
Merchant Marines	Yes	14	(14)	67	(3)	44	(9)
Post Office	Yes	14	(14)	33	(3)	25	(8)
Public Works	Yes	0	(13)	25	(4)	9	(11)
Rules	Yes	14	(14)	33	(3)	33	(9)
Science and Astronautics	Yes	0	(16)	100	(1)	33	(3)
Small Business	Yes	0	(2)	100	(1)	33	(3)
Standards	No	0	(10)	0	(1)	0	(3)
Un-American Activities	Yes	9	(11)	0	(2)	17	(6)
Veterans Affairs	No	7	(15)	0	(2)	0	(5)
Ways and Means	No	6	(16)	0	(1)	0	(3)

Source: Congressional Staff Directory, various years.

[a] Each entry gives the proportion of years, by committee and leadership change contingency, in which there was turnover of the staff director for the period 1962-78.

[b] TTB Hypothesis: Turnover occurs contemporaneously with, in anticipation of, or in response to committee leadership changes.

Table 7. Ties-That-Bind Hypothesis: Changes in Senate Staff Directors, 1962-78

		Mean Committee Turnover[a]					
	(1) Consistency with TTB Hypothesis[b]	(2) Years with No Chairperson Change		(3) Years with Chairperson Change		(4) Year of, Year before, Year after Chairperson Change	
		%	N	%	N	%	N
Aeronautics and Space	Yes	15	(13)	50	(2)	33	(6)
Agriculture	No	7	(15)	0	(1)	0	(3)
Appropriations	Yes	15	(13)	25	(4)	22	(9)
Armed Services	Yes	19	(16)	100	(1)	67	(3)
Banking and Currency	Yes	7	(15)	100	(2)	33	(6)
Budget	No	33	(3)	–	(0)	–	(0)
Commerce	Yes	19	(16)	100	(1)	100	(2)
District of Columbia	Yes	8	(13)	50	(2)	40	(5)
Finance	Yes	6	(16)	100	(1)	33	(3)
Foreign Relations	Yes	13	(16)	0	(1)	33	(3)
Government Affairs	Yes	7	(15)	100	(2)	40	(5)
Interior	Yes	6	(16)	100	(1)	33	(3)
Judiciary	No	18	(17)	–	(0)	–	(0)
Labor and Public Welfare	Yes	20	(15)	100	(2)	40	(5)
Post Office	Yes	15	(13)	50	(2)	17	(6)
Public Works	Yes	33	(15)	50	(2)	50	(6)
Rules	Yes	0	(14)	33	(3)	11	(9)
Veterans' Affairs	NA	–	–	–	–	–	–

Source: Congressional Staff Directory, various years.

[a]Each entry gives the proportion of years, by committee and leadership change contingency, in which there was turnover of the staff director for the period 1962-78.

[b]TTB Hypothesis: Turnover occurs contemporaneously with, in anticipation of, or in response to committee leadership changes.

this trend, especially in the post-1970 House, are (1) a decline in the power of institutional, party, and committee leaders, (2) the growth of subcommittees in both numbers and power, and (3) the more widely dispersed distribution of subcommittee chairmanships owing to Democratic Caucus rules. This growing autonomy of the individual member is found in the Senate as well, where most majority party members have come to possess multiple committee and subcommittee chairs. As well, there was a major reorganization of the Senate committee system in the mid-1970s. These institutional changes, in and of themselves, shake up staff relationships, thereby inducing turnover. But, in addition, they have secondary effects on other relationships. For example, *member* turnover of the mid-1960s among both Democrats and Republicans, though quite high, was far less consequential for committee staff turnover than the somewhat lower turnover of the 1970s (in the House, at least), since, during the latter decade, many more members had responsibility for hiring and firing committee staff.

The gist of this line of argument is that institutional change is a proxy for and/or sensitizes changes in factors and relationships which affect staff turnover. To examine this hypothesis, Table 8 arrays turnover statistics by committee and chamber, partitioning the data at the year 1970.[14] Thus, for the periods 1962-69 and 1970-78, we computed the mean committee staff turnover for each committee in each chamber. Although the differences in means are not always large, turnover during the decade of institutional flux (1970-78) exceeded that for the more institutionally quiescent period (1962-69) for 18 of the 20 House committees that existed in both periods, and for 14 of the 16 Senate committees. A more stringent test—that turnover in the latter period should exceed that in the former period by more than five percentage points—finds 13 of 20 committees in the House and 10 of 16 committees in the Senate compatible with the institutional change hypothesis. A parallel analysis for changes

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[14] We chose to break the data at 1970 since the 1960s were far more stable, in terms of change in institutional structure and practice, than the 1970s. The year 1970 is also conveniently the midpoint of our time series.

Table 8. Committee Staff Turnover (Percent)

Committee	House Mean Turnover		Senate Mean Turnover	
	1962-69	1970-78	1962-69	1970-78
Agriculture	14	18	0	23
Appropriations	7	22	3	17
Armed Services	8	10	7	18
Banking and Currency	29	34	12	25
District of Columbia	26	30	26	55
Education and Labor (Labor and Public Welfare)	25	15	24	42
Foreign Affairs (Foreign Relations)	4	26	23	14
Government Operations	9	24	8	32
Interior	10	20	26	27
Interstate and Foreign Commerce (Commerce)	17	19	18	34
Judiciary	12	22	15	13
Post Office and Civil Service	18	23	32	39
Public Works	12	20	18	22
Rules	13	24	23	25
Science and Astronautics	10	35	16	24
Ways and Means (Finance)	16	15	15	19
Veterans' Affairs	12	16	NA	38
Budget	NA	21		
House Administration	14	39		
Merchant Marine	9	19		
Small Business	NA	31		
Standards of Official Conduct	0*	19		
Un-American Activities	22	NA		

Source: Congressional Staff Directory, various years.

*Based on only two years.

in committee staff directors or chief clerks reveals that, in the House, staff director turnover in the period of flux exceeded that in the period of quiescence for 12 of 16 committees where there was a difference (there was no difference in four committees); in the Senate, the period of flux dominated the period of quiescence in 11 of 14 committees (two showed no difference).[15] In sum, there is reasonably strong support, though not without exceptions, for the institutional change hypothesis. Times of institutional rearranging do induce staff turnover.

Before turning to a discussion of the implications of staff turnover, we examine two final situational factors. The first—the partisan change hypothesis—argues that staff turnover is higher in years following a partisan change in the executive branch. Capitol Hill has long been a source for executive branch recruitment and, while such recruitment goes on continuously, it should be especially noticeable when a new administration is being staffed. From our data on committee staff turnover (tables available from authors), we compared mean turnover, by committee, in years of partisan change in the White House (1969, 1977) with that of other years in the 1962-78 period. For the House, the mean turnover in partisan change years exceeds that in other years for 20 of 21 committees (there is no difference for one and the comparison could not be made for another); in the Senate, this is true for 14 of 18 committees. Requiring a five-percentage point difference in order to support the hypothesis—the more stringent test—yields support for the partisan change hypothesis in 17 of 22 House committees and 12 of 18 Senate committees. By way of qualification, however, turnover in odd-numbered years tends to exceed that in even-numbered years, generally. Elections per se, whether midterm or presidential and whether involving a partisan change in executive or not, affect turnover in membership and hence in staff. Nevertheless, even though we cannot distinguish, with statistical confidence, between partisan change years and other post-election years, the high turnover of 1969 and 1977 reminds us that changes in the executive branch have impact upon and ripple throughout the entire Washington community.

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[15] We also examined leadership staff and subcommittee staff, though data collection on this latter group by *Congressional Directory* is not reliable since committee designations were so variable and idiosyncratic. In any event, the institutional change hypothesis is strongly supported there, too.

The last situational factor is chamber-specific. House and Senate committee staffs have different turnover rates. The S. Res. 60 arrangements (allowing senators to hire personal assistants with committee funds), coupled with a "life in the fast lane" feeling about Senate staff life, lead us to expect more turnover in the upper chamber. In order to examine the hypothesis of different turnover rates in the two chambers, we computed *mean committee half-lives*. For each committee, we examined yearly staff cohorts and determined how many years were required before half the cohort of the given year was no longer associated with the committee. Thus, for example, the 1962 House Agriculture Committee staff cohort was half-depleted sometime during 1967-1968; its half-life, consequently, was recorded as 5.5 years. Computing this number for each yearly cohort for which there were available data, and then further computing the mean half-life, we were able to rank-order House and Senate committee counterparts. Table 9 contains the results. For 13 of 17 paired committees, House mean half-life is higher. In only one of these instances is the difference less than a full year. In this sense, House full committee staffs would appear to be more stable and long-lasting.

Implications and Discussion

We have examined several hypotheses regarding congressional staff turnover against data that, while far from ideal, at least provide some insight about the impact on staff turnover of personal motivations, relationships between staffers and members, and situational factors, respectively. We have found significant but not wholly unambiguous support for the hypothesis that turnover results in part from the fact that many staffers look upon their Hill service as a credentialing experience and hope to fulfill their main life ambitions elsewhere. Secondly, we have found strong support for our ties-that-bind hypothesis as personal staffers stay or leave in close tandem with the members with whom they are primarily associated. Third, we found a more mixed picture in the case of committee staffs. In general, staffers are not simply birds of passage, especially those who work for committees; but neither are they as permanent as bureaucrats in the executive branch.

Institutional change within Congress has had important effects in expanding the number of

Table 9. Committee Staff Half-Lives, 1962-78

House		House-Senate	Senate	
Committee	Mean Half-Life*	Comparison	Committee	Mean Half-Life*
Armed Services	7.7	>	Armed Services	4.7
Public Works	6.4	>	Public Works	3.6
Ways and Means	6.0	>	Finance	3.7
Veterans' Affairs	5.9	>	Veterans' Affairs	1.9
Science and Astronautics	5.4	>	Space	3.3
Government Operations	5.4	>	Governmental Affairs	3.7
Foreign Affairs	5.4	>	Foreign Relations	4.9
Agriculture	5.4	<	Agriculture	7.0
Merchant Marine	4.8	–	NA	–
Commerce	4.5	>	Commerce	3.0
Interior	4.4	>	Interior	3.4
Post Office	4.3	>	Post Office	1.8
Rules	4.3	>	Rules	3.1
Education and Labor	3.9	>	Labor and Public Welfare	2.0
Judiciary	3.8	<	Judiciary	4.4
District of Columbia	3.4	>	District of Columbia	1.6
Ethics	2.8	–	NA	–
House Administration	2.7	–	NA	–
Appropriations	2.5	<	Appropriations	5.5
Banking and Currency	2.4	<	Banking and Currency	5.0
Budget	2.2	–	NA	–
Small Business	2.0	–	NA	–

Source: Congressional Staff Directory, various years.

*For each yearly staff cohort, by committee, the number of years required for 50 percent turnover was computed and then averaged over all yearly cohorts, 1962-1978.

staff positions and decentralizing the control of those positions, and these changes have led to increased turnover. We found also that staff turnover exhibited spurts following partisan turnover in the executive branch, and that generally the "fast track" of the Senate generated higher turnover rates than the House. Finally, we may note that all of these findings are reinforced by anecdotal evidence available in published form and are amply supported (as indeed they were often generated) by the extended interviews we have had during the past two years or so with staff members.

What are the consequences—for the operations of Congress and for the larger political system—of these rates and patterns of congressional staff turnover? First, what is the impact on the member of Congress? At the outset, we suggested that Congress has come to operate in a manner best understood as a collection of enterprises rather than as 535 discrete individuals. Each elected member hires and presides over the work of between 18 and 125 or so individuals, depending on chamber, state, and committee or subcommittee responsibilities. Necessarily, much of the time and energy of the members must be devoted to managing these enterprises, and, in a very real sense, the most direct effect of staff growth has been to transform the role of member from that of a legislator/politician into that of an executive manager.

What are the effects of staff turnover on these enterprises? Is it so rapid as to compound beyond endurance the management problems of the member, or render them sufficiently serious as to handicap severely the work of the enterprise; i.e., securing reelection and processing legislation? Our answer must be impressionistic but it is firm, nevertheless. Turnover among staff is not so great as to create severe management problems, and one important reason this is so is precisely why our data have shown support for the ties-that-bind hypothesis. Because staffers are hired by specific members and work under their direction, they soon come to accept a norm structure that emphasizes loyalty, of a very personal and specific kind, to be given to that member. Loyalty, in turn, implies a commitment to stay with the job, not necessarily forever or even until the member leaves, but for a "reasonable" tour of duty.

Not all staffers can accommodate themselves to this norm. Ideological commitment may dominate personal loyalty for some, and in such cases staff careers may be brief and turnover considerably higher. Serving a member of Congress is not the same thing as accomplishing a legislative agenda, though the one cannot usually be done except by means of the other. Staffers with deeply felt policy interests may find their Hill experience both frustrating and consuming, "burning out" rather quickly and returning to academe or moving to other arenas to continue their agitation. For others, the norm of loyalty is deeply personal, invested in a particular member, and not easily transferred. These staffers are observed to move in tandem with their member-patron. Finally, some staffers stay on for longer periods, surviving member turnover, partisan shifts and institutional change. This category of "professional" staff approximates the style envisioned by the sponsors of the 1946 LaFollette-Monroney Act (Galloway, 1953; Kammerer, 1951). It, too, entails some commitment to the norm of personal loyalty, but the commitment is more malleable and ultimately transferable.

We have argued that, in a member-centered legislative world, staff turnover is kept within tolerable limits by means of the development of and adherence to a norm of personal loyalty binding the staffer to his or her principal. The reciprocal of that relationship is the member's obligation to "take care of" a loyal staffer, facilitating internal advancement and extra-congressional career prospects and providing useful opportunities if necessary when the member leaves the arena. The consequence is that a sizable fraction of time and effort in the Washington world is devoted to career development and protection, by nearly everyone, to be sure, but specifically by members of Congress on behalf of their staffs. Indeed, some legislators are known for their staff networks spread throughout the Washington community; while formal ties are severed, member and ex-staffer nevertheless "keep in touch."

A further consequence of the fact that staff operate within member-centered enterprises is that disaggregation of effort, attention, and loyalties is accentuated. Rather than encouraging a focus on the party, the caucus, or even the committee, the mutual commitments of individual staffers to individual members means that on each issue consensus among both staffers and members must be *achieved* rather than emerging out of a common set of structural orientations. The dominant structural unit becomes the member-centered enterprise itself.[16] Members who are enmeshed in managerial responsibilities involving interactions with their staff are correspondingly insulated from interactions with other members. They are less likely to work closely with each other and hence less likely to develop norms and values in consequence of shared experience.

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[16] Even on the relatively consensual and "professional" House Committee on Appropriations, where staff is recruited by the full committee and its staff director, but for the most part is assigned to one of the 13 subcommittees, staffer loyalty is only nominally collective, to the full committee and the subcommittee. In large measure, staffer loyalty is to the particular subcommittee chairman. That this loyalty is easily transferable when a subcommittee chairmanship changes is a reflection of a particular kind of professionalism that marks both staff and members of this committee (Fenno, 1966). And in the Wilbur Mills years, the modest

On the other side of this coin is the active role of staffers themselves in the legislative process. One hears constant references in Washington circles to coalitions and settlements being negotiated by staffers in the name of their respective principals, and several observers have expressed alarm at the potential for abuse by these "unelected representatives" (Malbin, 1980), this "invisible force" (Fox and Hammond, 1977). It is very doubtful that much staff activity exceeds the bounds of member acceptance, but there is no doubt that a legislative process involving 535 enterprises with many thousands of individuals is very different from one in which the members act as unencumbered individuals.

If we look beyond Capitol Hill to the larger political community, staff growth and turnover has had one very important consequence. Congressional staff has become a kind of giant recruitment pool for our contemporary public life. Employment in the congressional milieu is comparatively open: no competitive examinations, no affirmative action, much spur-of-the-moment hiring. Executive agencies draw upon this source of people who, however innocent they were to start, are now experienced and knowledgeable in the ways of Washington. Interest groups do the same. So do law firms, public interest organizations, the media, and even academic institutions. Congress itself in 1980 contained nearly 60 members who once were members of congressional staffs. In a very important sense, the growth and turnover of congressional staff has served to provide an expanding pool of competent people to fill the myriad roles which modern public life requires. Former staffers provide a significant body of individuals who "know the system" at a relatively complex level and can serve as guides and counselors to those who do not yet possess that competence. Insofar as this is true—as congressional staff experience provides a basic apprenticeship for national public service in the United States—the functional significance of staff to the political order goes far beyond the traditional categories of discussion. Not only has its sheer size and its organization into member-centered enterprises transformed a nominally atomistic legislature of 535 individuals into an "industry" of nearly 1000 "political firms"; more significant, perhaps, is the fact that leaders in all walks of political life are increasingly learning the system from the vantage point of a congressional staff.

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staff of the House Ways and Means Committee, plus the staff of the Joint Committee on Internal Revenue Taxation, worked so completely for Mills, as did the other committee members for that matter, that disunity within the committee seldom appeared (Manley, 1968, 1970). Such domination of committees by their chairperson is largely a thing of the past, however, and it has become very difficult for staffers to serve a committee membership at large. Even the nominal collective loyalty on House Appropriations is sustained by the fact that staff tasks are highly routinized, and the responsibility of a "professional" is very like that of a "clerk" (which title, by the way, subcommittee staff directors on Appropriations bear).

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The Speaker's Task Force in the Post-Reform House of Representatives

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Because of changes in the issues at the center of controversy, changes in rules and norms and high membership turnover, the current House majority party leaders operate in a much less predictable environment than their predecessors did. In response to the changed environment, Speaker O'Neill has developed the strategy of leadership by inclusion, a central element of which is the Speaker's task force, an ad hoc group appointed by the Speaker and charged with passage of a specific bill. The leaders believe task forces help them perform both of their primary functions—building winning coalitions and "keeping peace in the family." By increasing the number of people working in an organized way to pass the bill at issue, the task force increases the probability of a bill's success on the floor. Work on a task force satisfies junior members' expectations of participation and fosters cooperative patterns of behavior among party members.

During the 1970s, changes in rules and norms, high membership turnover, and the rise to prominence of new party-fragmenting issues transformed the environment in which the majority-party leadership operates in the U.S. House of Representatives. The current leaders work in a much less predictable environment than their predecessors did.

Environmental change has not altered the functions which majority-party leaders are expected to perform. The leadership is charged with constructing winning coalitions on major legislation and thereby satisfying the legislative expectations of their membership and of significant actors outside the chamber. "Keeping peace in the family," as the current leaders themselves express it, is an equally important function. Centrally involved in peace-keeping are satisfying expectations of members about their individual roles in the chamber and fostering cooperative patterns of behavior among party members. The two functions are interrelated in a complex fashion. The need to promote harmony places limits upon the coalition-building strategies which can be used. On the other hand, to the extent that leaders are successful at "keeping the peace," their probability of future success in building winning coalitions is increased. The necessity of passing legislation which is controversial within the party inevitably strains party harmony. Yet, to the extent that leaders are sucessful at building winning coalitions on bills which satisfy their membership's expectations as to legislative results, they contribute to intra-party harmony.

Although the functions of leadership have not changed, successful performance requires strategies different from those appropriate in the pre-reform era. Scholars increasingly agree that congressional leadership is best understood from a contextual perspective (Jones, 1980; Cooper and Brady, 1980). The context or environment shapes and constrains leadership styles and strategies. The highly informal Rayburn style was predicated upon the power of the committee chairmen and the concomitant limited influence of rank-and-file members (Cooper and Brady, 1980; see also Ripley, 1967, 1969; Peabody, 1976; Jones, 1968). A strategy based upon bargaining with a small number of key actors obviously becomes untenable when the number of significant actors expands to include almost the entire House membership. Not only would such a strategy be unsuccessful in the short run, by thwarting members' expectations of full participation in the legislative process, it would also decrease the probability of future success.

In response to the changed environment, Speaker O'Neill has developed the strategy of leadership by inclusion. Effective leadership in the post-reform House, the Speaker believes, requires including as large a proportion of Democrats as possible in the coalition formation process. A central element of the strategy of inclusion is the Speaker's task force, an ad hoc group appointed by the Speaker and charged with passage of a specific bill. The leaders believe task forces have payoffs in terms of both of their central func-

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tions. By increasing the number of people working in an organized way to pass the bill at issue, the leaders increase the probability of success on the bill. Work on a task force gives junior members "a piece of the action" and satisfies their expectations of participation. In addition, the leaders believe that task force membership has a positive socializing effect, especially on junior members. Work on a task force gives them an understanding of the problems of the leadership, and, it is hoped, will teach them the value of joint action under the aegis of the leadership.

This study examines the origins of the task force concept, the type of bills on which task forces have been used, task force membership and how task forces operate. One purpose is to describe an as yet little-known leadership tool. A second is to illuminate the relationship between leadership strategies and the environment in which leaders work by analyzing how the leaders see task forces as aiding them in coalition building and as contributing to "keeping peace in the family."

The New Environment

An understanding of why new leadership strategies became necessary requires a brief review of the transformation of the environment in which the leadership works.

Rules Changes. The 1970s saw a constellation of rules changes aimed at curbing the power of committee chairmen and at dispersing positions of influence more widely throughout the House (see Ornstein, 1975; Dodd and Oppenheimer, 1977). Each member was limited to chairing only one subcommittee. Full committee chairmen were stripped of the power to select subcommittee chairmen and that power was vested in the Democratic caucus of the committee. Subcommittees were required to have fixed jurisdiction and adequate budget and staff. Full committee chairmen and the chairmen of Appropriations subcommittees were made subject to approval by secret ballot in the Democratic caucus.

The decade also saw an immense growth in the staff resources available to members of the House. In 1967, personal staff totaled 4,055; in 1979, that figure stood at 7,067. The number of employees of House committees rose from 702 in 1970 to 1,959 in 1979 (Bibby et al., 1980, pp. 69-70). The most junior member is now entitled to hire a staff of at least 18.

The decline in the power of committee chairmen, the dispersal of influence to the subcommittee level, and the augmentation of members' staff resources make wide participation in the legislative process possible. Another set of rules changes which can be summarized under the rubric of "sunshine reforms" further increase the incentives for participation. Except under special circumstances, all committee meetings, mark-up sessions as well as hearings, must be open to the public. Since the advent of the recorded teller vote, voting on amendments in the Committee of the Whole is no longer anonymous. The increased visibility of members' actions in committee and on the floor encourages the use of these arenas for making a variety of political points and subjects members to a wider range of pressures.

Membership Turnover. Membership turnover in the 1970s has been very high by post-World War II standards. The number of Democratic freshmen and the proportion of Democratic members who were freshmen was unusually high in the 94th, 95th and 96th Congresses. The 75 freshmen elected in 1974 in the wake of the Watergate scandal constituted a little more than a quarter of the Democratic membership during that Congress. The 47-member Democratic freshmen class elected to the 95th Congress and the 42-member class elected to the 96th were larger than any class between 1960 and 1972, except for the 89th Congress class elected in the 1964 landslide. The result of this heavy turnover has been an increasingly junior Democratic membership. In the 94th Congress, 44 percent of Democrats were serving their first, second, or third term; that figure rose to 51 percent in the 95th and to 53 percent in the 96th. In contrast, between 1961 and 1974, the mean percentage of first-, second- and third-termers was only 31 percent; its highest value was 42 percent in the 89th Congress.

For the leadership, the sheer number of newcomers presents a problem, since the rules now make possible full participation by the most junior member. The leaders obviously lack longstanding relationships with these members. Yet for the leadership such relationships are crucial for making informed judgments about the probable behavior of members and provide the basis for effective persuasion.

Changes in Norms. Accompanying the change in rules and membership has been a change in norms. Socialized to politics during the turbulent 1960s, the freshmen of the 1970s entered the House in a period of challenge to the power structure; many of the large '74 Democratic class were elected from previously Republican districts. Both personal inclination and political necessity dictated a high level of activity and immediate participation at the committee and the floor stage. Rules changes which lessened the control of seniority leaders and augmented the staff resources of junior members made such activity possible. Thus norms of apprenticeship and of deference to senior members, already weakened during the 1960s, were given a final blow.

The events of the 1970s have diminished the prestige of the committees and undermined the norm of reciprocity among committees. Previously, there existed a strong presumption in favor of the committee's bill. "Go with the committee on the floor" with the watchword; "the committee has exhaustively examined the matter and has brought its expertise to bear." The reciprocity norm is much weaker now, and there is a much greater tendency to "legislate on the floor." Younger members are simply not willing to defer to committees.

New Issues. The issues at the center of controversy in the 1970s and early 1980s—energy, environmental and consumer issues, and foreign and defense policy questions—split not just North from South in the Democratic party; they also frequently divide northern Democrats (Sinclair, 1981). High inflation has engendered a budget-cutting mood which threatens to destroy the northern Democratic consensus on traditional economic and social welfare policy. Socio-cultural issues such as busing and abortion continue to excite controversy.

Because of the change in floor procedure, many of the divisive issues come to a recorded vote over and over during a Congress. The highly emotional abortion issue is a good example. Anti-abortion members are proposing amendments barring federal funding to more and more bills and are forcing roll-call votes. The frequency with which members are compelled to take public stands on such highly charged issues cannot help but exacerbate the leadership's problems.

Impact upon Party Leadership and the Strategy of Inclusion

The changes of the 1970s have resulted in a much less predictable House environment. According to a top leadership figure, "the democratization of the House has proceeded so far that all kinds of things can come percolating up from below and that can cause problems for the leadership."

The recorded teller vote and the decline of intercommittee reciprocity have severely heightened unpredictability at the floor stage of a bill. Since neither junior status nor party affiliation nor lack of expertise bars members from offering amendments, amendments can come from anywhere; it is impossible to anticipate all that may be offered. The House, according to Morris Udall (D-Ariz.), has become a "fast breeder reactor" for amendments. "Every morning when I come to my office, I find that there are 20 new amendments. We dispose of 20 or 25 amendments and it breeds 20 more amendments" (*Congressional Quarterly,* p. 877). The number of recorded votes grew manyfold during the 1970s. During the 91st Congress, the last before the institution of the recorded teller vote, the House took 443 recorded votes; during the 95th Congress (1977-78), 1540 recorded votes were taken.

As a vote can be recorded upon request by 25 members—20 until 1979—almost any member can force a vote. Republicans have become very adept at fashioning amendments with considerable constituency appeal. Votes which a clever opponent back home can represent as favoring reparations to Vietnam, "giving away" the Panama Canal, or supporting federally funded legal aid for homosexuals present a ticklish choice even to the Democrat who would like to support the leadership.

The large number of amendments offered to many major bills strains the leadership's resources and increases the probability of the committee majority and the leadership getting "ambushed" on the floor. "We are in the process of moving from a time in the history of the House when committees had too much power, and we are moving to a point where the floor itself becomes a large, unwieldy committee," according to Paul Simon, D-Ill., "We are not doing the job of legislating as we should. We are legislating by whim. . ." (*Congressional Quarterly,* p. 878).

The heightened level of floor activity is even more of a problem than it might be otherwise, because voting behavior has become less predictable. The leadership cannot count on solid support from northern Democrats as it could on most issues during the 1960s. On the other hand, the southern Democratic contingent in the House became more heterogeneous during the 1970s. As the race issue declined in salience in the South, somewhat more nationally oriented Democrats were elected. Consequently, when the party leadership attempts to build winning coalitions, there are few Democrats who can be written off completely and few who can be counted on to be loyal across the board (Sinclair, 1981).

The Speaker's response to heightened unpredictability is the strategy of inclusion. O'Neill sees effective leadership in the post-reform House as dependent upon including as many Democrats as possible in the coalition formation process. Coping with the decrease in predictability requires more extensive vote gathering. A larger proportion of the Democratic membership must be contacted and persuaded—and this task must be done many times. The task is simply too great to be handled solely by the top leadership. Others must be brought into the process.

The leadership could and, in many cases, does rely upon the whip system for help. During the 1970s, the whip system significantly expanded in size; by 1977, it consisted of the majority whip, a chief deputy whip, three deputy whips, 22 regionally elected whips, and 10 at-large whips. Several factors militate against exclusive reliance on the whip system for aid in coalition building. The whip system is charged with conducting whip polls and, as the number of polls has increased, this has become a time-consuming task. In the 95th Congress, about 80 whip counts were conducted. In addition, because the regional whips are elected by the members of their zones, their loyalty to party and to the leadership is sometimes minimal. The addition of at-large whips, appointed by the leadership, was a response to the problematical loyalty of the regional whips. The zone whips are expected only to do the counting; the appointed whips do act as an arm of the leadership in mobilizing support.

O'Neill's strategy of leadership by inclusion is not predicated only upon the leadership's need for a loyal group of assistants to aid in coalition building. It also encompasses satisfying members' —especially junior members'—desires for participation, for "a piece of the action." If junior Democrats are involved in the vote-mobilization process via task force membership, their very considerable energies are directed towards accomplishing leadership objectives and they are given a stake in the leadership's success. This involvement, it is hoped, will gradually teach junior members the need for "followership" and the value of joint action under the aegis of the leadership.

Origins of the Task Force Concept

The basic notion underlying the task force concept—that of joint organized effort to pass a specific bill—is, of course, not new. As a senior staffer explained: "People have been doing task forces of one kind or another for a hundred years or more. Any time that there's an interested group of people, they're going to get together and work on it in some kind of common front."

Task forces consisting of interest-group representatives have for years worked with the party leadership on legislation of common interest. The leadership has traditionally worked closely with committee leaders in passing major legislation and, during the early 1970s as the number of significant actors increased, the leadership seems to have expanded the number of members included in its vote-mobilization efforts.

The informality of such traditional arrangements distinguishes them from the current Speaker's task force. A senior staffer closely involved in the process explained: "In the sense that it's now defined, which is a pretty structured kind of thing, with a chairman appointed by the Speaker and a list of members, I guess you'd have to say that it started in the 95th Congress." He continued: "We have some tough bills. We had one that we lost and we lost it because we didn't do one or two things we could have done. I remain convinced to this day that we had the votes if we had played our cards right but it would have taken organization to do that."

An early loss which the leadership attributed to lack of organization acted as the catalyst. The loss taught the new leadership team that building winning coalitions in the post-reform House requires more organized and extensive vote-gathering efforts than were needed in the pre-reform era.

The Decision to Establish a Task Force

The decision to appoint a task force is solely the Speaker's, although he may do so upon the advice of other leadership figures or of staff. He will, of course, confer with the prospective floor manager of the legislation.

Asked about the types of bills for which task forces will be formed, a senior staffer explained:

> They are significant leadership issues where the Speaker has some prestige on the line, and, much more importantly than that, where the core of the party has to have the bill. Now, that's pretty easy to pick out. You look down the list of bills which came up in the Congress and you'll find the 8 or 10 bills that really had to pass last year—you know, we had to have the budget, we had to have the debt ceiling—some elements of the party had to have Humphrey-Hawkins; CETA comes in the same bag; energy, the Democratic party had to have; a tax bill we had to have of some sort—it isn't the tax bill that we would have liked to have, but we had to have it; and you look down the list of other bills, and I would guess that there aren't any that if they had not passed they would have done as much damage to the party or to the leadership or to the House, the Democratic members of the House, as those bills if they had not gone through, so that's how it's done.

Another staffer made the same point in the imperative voice: "[Task forces] should be used only on significant legislation that is clearly part of the Democratic party program." An important consideration, of course, is "how deep the trouble is." Table 1 lists all task forces appointed through the end of 1979. All the bills are ones on which trouble was expected and ones which the Democratic party as a whole or some significant segment thereof "had to have."

President Carter's top domestic priority during the 95th Congress was a comprehensive energy program. His prestige and that of the party and the leadership rode on success in passing energy legislation. Energy policy deeply splits congressional Democrats along consumer versus producer lines; consequently, the leadership knew passage would be difficult. The Energy Organization Act is the first bill on which a full-fledged task force was appointed. It actually consisted of three subgroupings, each charged with defeating or passing a key amendment.

The Senate drastically changed the energy legislation so as to make final House approval more difficult. Liberals were extremely upset by the gas-pricing section and House approval of the conference report rested upon passage of a rule which prevented a separate vote on that section. The ex-

Table 1. Speaker's Task Forces, 1977-1979

Task Force	Chair	House Vote	Democratic Vote	Date
Energy Organization Act (HR8444)				
Natural Gas	Sharp, Ind.	199-227	72-210	8/3/77
Plowback	Gibbons, Fla.	198-223	74-208	8/4/77
User Fees	Mikva, Ill.	221-198	212- 68	8/5/77
Passage	Sharp, Ind.	244-177	231- 50	8/5/77
Social Security (HR9346)				
Fisher Amendment	Fisher, Va.	386- 38	257- 24	10/26/77
Pickle Amendment	Tucker, Ark.	196-221	74-206	10/26/77
Passage	Gephardt, Mo.-Tucker	275-146	235- 46	10/27/77
Conference Report	Gephardt-Tucker	189-163	174- 54	12/15/77
Humphrey-Hawkins (HR50)	Rose, N.C.	252-152	233- 41	3/16/78
1st Budget (S. Con. Res. 80)	Derrick, S.C.	201-197	198- 61	5/10/78
Debt Ceiling (HR13385)	Gephardt, Mo.	205-202	196- 74	7/19/78
2nd Budget (H. Con. Res. 683)	Derrick, S.C.	217-178	215- 42	9/16/78
CETA (HR12452)	Miller, Calif.-Oberstar, Minn.-Mineta, Calif.			
Obey Amendment		230-175	114-152	8/9/78
Jeffords Amendment		221-181	101-162	8/9/78
Passage		284- 50	206- 18	9/22/78
Tax (HR13511)	Gephardt, Mo.			
Kemp Amendment		177-240	37-237	8/10/78
Passage		362- 49	224- 47	8/10/78
Energy (HR5289, HR5037, HR5263, HR5146, HR4018)	Sharp, Ind.			
Previous Question on Rule		207-206	199- 79	10/13/78
Adoption of Conference Report		231-168	185- 81	10/15/78
Debt Limit (HR1894, HR2534)	Gephardt, Mo.			
Previous Question on Rule		222-197	221- 44	2/28/79
Passage		194-222	191- 73	2/28/79
Previous Question on Rule		201-199	200- 54	3/15/79
Passage		212-195	209- 53	3/15/79
1st Budget (H. Con. Res. 107)	Mineta, Calif.			
Passage		220-184	211- 50	5/14/79
Conference Report		144-260	108-152	5/23/79
Conference Report		202-196	174- 80	5/24/79
2nd Budget (H. Con. Res. 186, S. Con. Res. 36)	Mineta, Calif.			
Passage		192-213	188- 67	9/19/79
Passage		212-206	212- 52	9/27/79
Chrysler Aid (HR5860)	Blanchard, Mich.	271-136	209- 48	12/18/79

Source: Compiled by the author from task force membership lists.

pected and actual closeness of that vote made an organized task force effort necessary.

Both the presidential and the party leadership considered legislation to place the social security system on a sound financial footing among the top priority items of the 95th Congress. The steep increase in social security taxes that this entailed resulted in a far from popular bill. The social security task force consisted of subgroups charged with responsibility for specific amendments. The Fisher task force was charged with passing an amendment deleting mandatory coverage of federal workers. The overwhelming passage of the amendment makes that task force something of an anomaly. A staffer who worked with the task force explained:

> The preliminary counts didn't show anything like that. That's part of it, but if you'd asked anyone around here, "Is it going to pass?," we'd have said yes. But the crucial thing is, what if it hadn't? We had no social security bill plain and simple. It had to pass and, while most people would say the chances are 9 out of 10, it was not a situation where one was willing to take that 10-percent risk.

The steep increase in social security taxes, which the social security bill mandated, made it politically advisable to pass a tax reduction bill before the 1978 elections. That some bill would pass was never in doubt; the form of the legislation was at issue. Representative Jack Kemp and Senator William Roth had received a great deal of publicity for their proposal to cut income taxes by 30 percent and the leadership knew Kemp would offer the proposal as an amendment to the tax bill. Its obvious appeal, combined with what the leadership saw as drastic consequences if it should pass, led to the establishment of a task force charged specifically with defeating the Kemp amendment.

The reputation of the House Democratic leadership rests, in part, on its success in gaining passage of presidential priorities. It also depends upon the leadership's ability to uphold the reputation of the House as a functioning legislature on less highly visible matters. Two areas that have been especially problematic for the leadership are the biannual conflicts over the budget resolution and the recurrent fight to raise the federal debt ceiling. In a time of high inflation, building winning coalitions in these areas has become increasingly difficult. Both budget resolutions and debt-limit votes offer Congress members nearly irresistible opportunities for "grandstanding." An intensive and well-organized vote-mobilization effort on such legislation has become a prerequisite to success.

The remaining bills on which task forces were established were centrally important to major segments of the Democratic membership. Big city liberals needed the Humphrey-Hawkins Bill and the CETA bill and both seemed to be in deep trouble. Michigan Democrats as well as some others with Chrysler plants in their districts considered the Chrysler bail-out bill "a matter of life or death." Nevertheless this bill, because of its relatively narrow range, fits the criteria least well. The Michigan Democratic delegation, it was reported, "begged" the Speaker to establish a task force.

The technique, then, has generally been restricted to significant legislation which involved the prestige of the leadership and of the party as a collectivity or bills which a major segment of the party badly needed. Restricting the use of task forces to such legislation is vital if the strategy is to contribute to maintaining harmony. Use on a bill perceived by a major segment of the party as threatening its vital interests would undermine the legitimacy of the technique. Instead of teaching members the value of joint action under the aegis of the leadership, it would exacerbate intraparty conflicts.

Task Force Membership

Once the decision to establish a task force has been made, the Speaker appoints the chairman. Leadership staff may suggest one or several names, or he may ask the chairman of the committee, "Who's a bright young guy who would do a good job on this?" As Table 1 shows, the chairman is almost invariably a junior member. All task forces but the first were chaired by members first elected in 1972 or later. Dick Gephardt of Missouri chaired several task forces during his first term.

Junior members are chosen, in part, because they have more time to devote to the task than senior members with heavier committee responsibility. In addition, giving junior members an important job to do and drawing them into the leadership's orbit are integral aspects of the strategy of inclusion. One of the leaders said that an effort was made "to pick members who will stay around, who intend to make a career of the House." The chairman is usually, though not invariably, a member of the originating committee. The members chosen to chair task forces are otherwise distinguished from their contemporaries not by objective characteristics such as prior political experience but by the leaders' perception of them as responsible legislators and as leaders among their peers.

The Speaker frequently suggests some of the members of the task force, but also gives the chairman considerable discretion to include "anyone else you feel comfortable working with." In selecting members, the task force chairman works with the Steering and Policy Committee staff, who provide staff work for the task forces. The selection process has become rather elaborate. Prior to meeting with the task force chairman, the staff prepares a list of potential members with information on each. The information includes past support on similar legislation. Commitment to the legislation in question is, of course, a prerequisite to membership. Within that constraint, attention is paid to getting "regional, seniority and ideological balance."

An examination of task force membership will show the extent to which the aims of including a large number and variety of Democrats in the process and especially of getting junior members involved are actually met. The analysis is based upon membership lists of all 95th Congress task forces and the first two task forces established during the 96th.

Although broad inclusion is an objective of the task force concept, passing the bill at issue is paramount. Task forces must be kept at a working-group size. The 11 task forces under discussion averaged 28.5 members; they ranged from the 16-member CETA task force to the Social Security task force and the Energy Conference task force, each of which had 41 members.

Table 2 shows the extent to which the objective of broad inclusion was reached. During the 95th Congress, 116 members—40.3 percent of the Democrats—served on at least one task force. The table also shows that there was considerable membership overlap among task forces. One in five Democrats served on two or more task forces during the congress; one member, in fact, served on each of the nine in the 95th and on the first two in the 96th.

The strategy of inclusion refers not only to getting a large number of members involved but also to including members from all sections of the party. Overall, the task forces have been fairly representative in terms of region. In both the 95th and 96th Congresses, 74 percent of the Democratic membership was from the North and 26 percent from the South.[1] The average northern membership across the 11 task forces was 76.1 percent; the average southern membership, 23.9 percent. The regional balance varied, but only one task force was clearly unrepresentative: no southerner served on the CETA task force. The unpopularity of the program seems to have made it impossible to find southerners willing to work for the leadership position on the bill.

Table 2 presents data on the proportion of northerners and southerners who served on task forces during the 95th Congress. Southerners were less likely to have served than northern Democrats, but the differences are not massive. The leadership has done reasonably well at including southern Democrats in the coalition-building process via task force membership. In fact, a larger proportion of southern than of northern Democrats served on two or more task forces during the 95th Congress. Clearly, those southerners willing to participate get plenty of opportunity to do so.

The requirement that task force members be committed to passing the bill in question prohibits strict ideological balance just as it makes absolute regional balance somewhat difficult. As one would expect, task force members as a group are more loyal to party than non-task force members. Considering each of the 11 task forces separately, one finds that in each case, members have a higher average party unity score than nonmembers and that this pattern holds for both of the regional groupings.[2] The differences between members and nonmembers are especially great among southern Democrats.

Table 2. Regional Representation on Task Forces, 95th Congress

No. of Task Forces Member Has Served On	All Democrats	Northern	Southern
0	59.7	58.7	62.7
1	19.4	22.1	12.0
2	8.7	6.6	14.7
3 or more	12.2	12.7	10.1
N =	288	213	75

Source: Compiled by the author from task force membership lists.

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[1] Members from Alabama, Arkansas, Florida, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Texas and Virginia are classified as southern, all others as northern.

[2] *Congressional Quarterly* party unity scores are used (*Weekly Report*, December 16, 1978, pp. 3450-51). Scores have been adjusted so that absences do not affect the scores.

Table 3 shows that Democrats who served on one task force during the 95th Congress are, as a group, appreciably more loyal than those who served on none. Those serving on two or more are slightly more loyal still. The differences are especially marked for southern Democrats. Nevertheless, task force members are relatively heterogeneous ideologically. Southern Democrats who served on two or more task forces are, as a group, significantly less loyal than northern Democrats who served on none. The leadership does find it possible on specific bills to involve members who otherwise frequently defect. A few Democrats who are generally highly disloyal have served on task forces.

An interesting outgrowth of the strategy of inclusion and an indicator of the decline in committee insulation in the House is the committee makeup of the task force membership. Although the chair usually is a member of the originating committee, a majority of the task force membership is not. If the two energy task forces for which the figures would not be meaningful are excluded,[3] then on the average, only 30.6 percent of task force members are also members of the committee reporting the bill.

The subgroup of members of the Social Security task force charged with passing the Fisher amendment included a number drawn from the Post Office and Civil Service Committee. Since the Fisher amendment dealt with federal workers and thus was related to that committee's jurisdiction, the inclusion of Post Office and Civil Service Committee members on the task force was a way of recognizing their legitimate interest in that aspect of the bill. That case is, however, the exception. On other task forces, members not from the originating committee held positions on a wide variety of committees. They were simply Democrats with an interest in and a willingness to work for the bill in question.

A primary aim of the task force concept in getting junior members involved in the coalition-building process. That objective has clearly been accomplished. During the 95th Congress, 51.7 percent of the Democratic membership had begun its service in 1973 or later; on the average, 69.4 percent of the members of the 9 task forces was drawn from this junior group. Democrats first elected in 1972 through 1976 accounted for 45.8 percent of the party's members in the 96th Congress and for 60.1 percent of the membership of the first two task forces. Freshmen constituted 15.2 percent of the Democratic membership and 18.5 percent of those serving on the two earliest task forces. The first task force of the 96th Congress was set up only a month after Congress convened, yet even in this case freshmen were included; 12.1 percent of the debt limit task force membership consisted of freshmen. On the budget task force established in late spring, freshmen constituted 25 percent of the membership.

Table 4 shows that, during the 95th Congress, slightly over half the Democrats first elected in 1972 or later served on at least one task force. Junior members were also more likely than their senior colleagues to be frequent task force participants.

The leadership has been successful in getting a large proportion of Democrats generally, and of junior members specifically, involved in task force efforts. Membership has been drawn fairly proportionately from both northern and southern

Table 3. Relationship of Party Unity to Frequency of Task Force Membership and Region, 95th Congress

Group	Frequency of Task Force Membership		
	None	One	Two or More
All Democrats	68.4	78.4	79.6
N =	(171)	(56)	(60)
Northern	77.0	81.2	84.9
N =	(124)	(47)	(41)
Southern	45.6	63.8	68.2
N =	(47)	(9)	(19)

Sources: Compiled by the author from task force membership lists and from *Congressional Quarterly*'s party unity scores, *Weekly Report*, December 16, 1978, pp. 3450-51. Scores have been adjusted so that absences do not affect the scores.

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[3] The energy bill was referred to five standing committees and to the Ad Hoc Energy Committee. Of the members of the first energy task force, 64.3 percent were from Ways and Means or Commerce, the two committees with major jurisdiction, and 44.4 percent from the Ad Hoc Committee. On the second energy task force, 22 percent were from Ways and Means or Commerce and 26.8 percent from the ad hoc panel.

Table 4. Relationship Between Seniority and Frequency of Task Force Membership, 95th Congress

No. of Task Forces Member Has Served On	Senior Members	Junior Members*
0	70.5	49.7
1	16.5	22.1
2	5.8	11.4
3 or more	7.2	16.8
N =	139	149

Source: Compiled by the author from task force membership lists.

*First elected in 1972 or later.

members and, to some extent, from conservatives as well as liberals. From the leadership's point of view, broad inclusion is important because it contributes to the successful performance of both central leadership functions. A broadly representative task force is more likely to be successful in passing the legislation at issue because it includes members with ties to all segments of the party. Giving a large number and a wide variety of Democrats "a piece of the action" but under the aegis of leadership, it is hoped, will foster norms of intraparty cooperation.

The Task Force in Action

The broad strategy to be pursued on a bill often evolves out of what has happened during committee deliberations. On a bill important enough to warrant a task force effort, the party leadership will meet with key committee leaders to discuss strategy. According to a leadership aide, "The chairman of the task force is centrally involved."

Strategy, of course, varies. A participant explained that on the first budget resolution in 1979, "We had made up a careful package in committee. The strategy on the floor was to sell it as a carefully crafted package and beat back all [non-committee sponsored] amendments." A package amendment adding funds for various social programs, as well as for two destroyers ordered and then canceled by Iran, was brought to the floor by the committee. According to a senior staff member, "Bringing the additions in the Simon amendment to the floor rather than adding them in committee was a conscious strategy. The purpose was to reinforce the notion of a package."

A similar strategy was used on the energy bill. It too was sold as a carefully crafted and balanced package. On Humphrey-Hawkins, it was decided that intense effort should go into defeating amendments because, in the words of a participant: "All you had to do was have one big bad amendment and the bill was finished. Because too many didn't want to vote for it anyway that, if you gave them a chance to say, well, it's got this bad amendment on it now—no bill. You had to win every one." The social security bill reported out of Ways and Means made participation by government workers mandatory, a provision the leadership considered politically untenable. Consequently, the leadership decided that an amendment deleting the provision would be offered on the floor.

Typically, the first task force meeting begins with a "pep talk" by the Speaker. The Speaker, the majority leader and the majority whip are ex officio members of every task force. Their frequency of attendance at meetings depends upon just how much trouble they believe the bill to be in, personal interest, and the absence of conflicting commitments. The Speaker was reported to have attended every meeting of the budget resolution task force in the spring of 1979. When the leaders cannot themselves attend, they send top aides in their stead.

Usually, the task force will begin with a whip count done by the whip system. Because, in the post-reform House, many amendments are offered, the counting process has become much more complicated. The whip system conducts polls on many bills and, consequently, the questions asked by the whips on a given bill must be restricted in number and fairly simple. According to a leadership aide:

> Task forces are especially helpful if the issues involved are very complicated substantively and you really need to run an information campaign to inform members. And where there are lots of possible permutations and you need to really get a feel for the members—that is, what they'll do under a variety of circumstances.

At the first meeting, strategy will be discussed and the task force members will be assigned a list of Democrats "they will be responsible for working [on] throughout" consideration of the bill. A task force chairman explained the process in a case in which no whip poll had been conducted:

> It was just a question of dividing up the states and assigning those members to some specific person. For instance, in the state of California, we had three people on the task force; from New York, I believe we had two. And from Texas, of course, we had Mr. Wright as well ______. Mr. ______ took Pennsylvania and also West Virginia because he's such a good friend of ______ and ______. So we were able to divide among the members of the task force all the Democrats and so then we had to decide what was the critical issue. We knew we were going to have a series of amendments, but I didn't want at the start to

> have to give 7 or 10 questions to a person to have to ask and so, in our discussion in the task force, we reduced it finally to two.

When the zone whips conduct a whip poll, they are simply counting. For task force members, counting and persuasion are always combined. Consequently, the process differs only slightly if the task force begins with a whip poll. According to a leadership figure:

> We'll kick around arguments—discuss which ones will be good with which members. Then we'll go through the whip poll and assign names. The list of names will be read, and a member will say, "I'll take that one." Or maybe no one will volunteer and someone will say, "______, why don't you take him?" and ______ will say, "I don't want to." But finally all the names will be assigned.

Or, as a senior staffer expressed it, "There will be candid discussion until they pin down where the trigger is."

Region and seniority are the primary bases for assigning names because, according to the participants questioned, they are the primary bases upon which friendships form. The strong belief that members will be more successful at persuading their contemporaries provides an additional reason for the heavily junior membership on the task forces. As in other vote-mobilization efforts, finding the "trigger" may mean going outside the House. According to a staffer, one of the ways of "motivating" members is via outside groups: "To get ______'s vote, you get Kenny Young [of the AFL-CIO] to call him."

The task force will, when possible, work with friendly interest groups. The chairman or the Steering and Policy Committee staffer working with the tassk force may meet with interest-group representatives to coordinate strategy. Such meetings on the Humphrey-Hawkins Bill involved about 40 interest groups. Information on voting intentions which such groups have gathered allows the task force to double-check its data on Democrats and to get some reading on Republican votes.

Although the results of whip polls are never given out, some trading of information with the Carter administration did occur and Bill Cable, White House liaison with the House, was sometimes invited to task force meetings. But, according to a leadership aide: "[The White House liaison people] will not usually attend the strategy meetings. They will talk to the Speaker separately. They are usually content to leave the details to the House leadership. They will lobby individual members themselves."

The arguments used by task force members as they attempt to persuade their colleagues vary. On the first budget resolution in 1979, the carefully balanced nature of the committee product and party pride were emphasized. A member explained: "Our strategy was that this was basically a majority party document. This was written by the budget committee majority, and we ought not let the minority party dictate what the majority party is going to pass." On debt-limit votes: "You ridicule the opposition arguments, minimize the electoral impact and appeal to their sense of responsibility."

Arguments about the substance of the matter at issue generally play a major role. A budget task force member commented: "There were some of these amendments that were very appealing considering the Prop. 13 mentality. So we had to really explain to them why we ought not to be succumbing to this kind of thing." The most persuasive argument varies from member to member. A participant remarked:

> It's very individualistic—you go by instinct. Members have different motivations. Some you have to puff up. ______: you tell him he'll get credit, that he's a leader. ______: you persuade him he's being patriotic. Others, you persuade them it's the devious thing to do politically. Some you have to convince it's the liberal thing to do; others, that it's conservative.

When a member cannot be persuaded to support the leadership position across the board, the task force will work for partial support. A participant describes this strategy as used on the first budget resolution in 1979:

> Now, part of that frankly was sold on the basis of, "If you're going to vote no on final passage of the budget resolution, help us out at this point by voting no on Holt-Regula or any other Republican substitute." And that argument seemed to hold with a lot of people, that they were willing to help us out by voting no on any Republican substitute, even if they were going to vote no on the resolution. The case of Texas, as an example. Everyone of them voted no on both Latta and Holt-Regula and that really helped us out immensely.

Several days after the first meeting, the task force will meet again and discuss the results. The names of Democrats now committed to voting with the leadership are marked off, and the remaining names may be reshuffled. The task force may go through this process several times. The top leaders always take some names. One of the purposes of setting up a task force, however, is to minimize the number of members whom the top leaders must persuade. They are busy and their personal appeals for votes are most likely to be effective if not made too frequently. Consequently, an attempt is made to save the top leadership, especially the Speaker, for "the tough ones."

The floor manager of the legislation is always invited to task force meetings. His attendance seems to depend primarily upon time pressures. Asked about the relationship among the party leadership, the floor manager and the task force, a top leadership figure said, "The leadership is the general staff, the floor manager, the battlefield general and the task force, the company commanders." The floor manager and the task force chairmen do seem to work together smoothly. Generally, floor managers seem to appreciate the help the task force provides, and the likely working relationship between the task force chairman and the floor manager is probably an important consideration in the choice of the task force chairman. A task force chairman, discussing his working relationship with the committee chairman, said:

> Well, of course, Chairman ______ sat in on every one of these sessions. So he saw whatever strategy we were developing within the task force and could say, "Hey, I don't think you ought to be doing that." Whenever I had a chance before going into the meetings, I'd try to check with him on certain things. I'd say, "______, this is what we're thinking about doing; what do you think?" He'd say yes or no.

Much of the task force's activity takes place during the period of floor consideration. Because numerous amendments are now proposed to most controversial bills, the floor stage has become much less predictable. It is not possible to count every amendment before the bill reaches the floor, both because of the numbers involved and because some will be offered without prior warning. Amendments can be extremely complicated and their actual effects obscure. Consequently, although vote-mobilization efforts in the days before a bill reaches the floor are essential, holding the coalition together during floor consideration and warding off amendments which could seriously damage the bill are equally crucial.

The task force chairman stays on the floor during the entire period of consideration. Members are, at minimum, expected to be on the floor during each roll call. Standing at the doors, task force members hand out information on the amendment pending. They are expected to keep track of the people assigned to them and work on them during a roll call if necessary. The task force chairman and some of the members "cluster around the monitor and look at the vote state by state. If they find someone has voted wrong, they will chase him down and work on him. Even if he comes to the floor, votes and then runs, he's got to be around somewhere and they will find him." Other task force members are charged with keeping pocket votes—those Democrats who have promised to vote with the leadership if they are needed—from leaving the floor. A female task force member was reported to have kept her assignee from leaving the floor by "practically sitting in his lap. He would have had to be a real boor to have gotten up."

The top leadership will also stay on the floor during important votes. This gives the leaders an opportunity to engage in some last minute persuasion and their presence signals the importance they place upon the vote. When the Speaker stands at the monitor examining how members voted, Democrats know "he's really watching this one. He wants it."

The unpredictability of events at the floor stage puts a premium on being able to respond quickly. When things go wrong, remedial action must often be taken immediately to prevent the supportive coalition from disintegrating. Early in the consideration of the budget resolution in May 1979 an amendment cutting $250 million of targeted fiscal assistance passed. If the decision had been allowed to stand, it would have split the coalition supporting the resolution and made the adoption of other major amendments more likely. A participant explained:

> We were using the argument, "Look, this is a very carefully written budget resolution to make sure we're satisfying the agriculturalists, the urban community, the rural, the East, the West, the whole thing, and don't monkey with this very tenuous balance we have." As soon as that $250 million was taken out, you could hear people saying, "Well, the hell with it; I'm not going to vote for this damn thing."

The committee chairman immediately met with a group of his members and with Budget Committee staff to draft a substitute which would restore $200 million. At this point an organized working group was crucial. The task force chair explained:

> Once we had [the Solarz amendment], we got to work. We recalled everybody and said "All right, here's what's happening, we need the votes on this one. Make sure the people you have as your responsibility vote yes on the Solarz amendment." So, within two days we came right back and got that thing reversed and got the $200 million added in.

Because of the number of amendments offered and the availability of recorded votes, an effective method of providing information to members has become necessary. Members simply will not blindly follow the leadership over and over again, especially when their votes are public. Thus the development of the "bullet"—a mimeographed, one-page summary of an amendment. Developed by the Steering and Policy Committee staffer working with the Humphrey-Hawkins task force, bullets have now become a regular part of the process. For anticipated amendments, the committee staff will prepare bullets prior to floor consideration. On amendments offered without warning, bullets will have to be written and duplicated on the spot. The committee and the Steering and Policy staff work together, often using an Appropriations Committee room close to the House floor.

Bullets are widely believed to be effective. A staffer said:

> On bad amendments, you need something to give the members. You've got to give them a rationale for voting against. Otherwise, you can have the "lurch" effect. If they have nothing and they go in, they'll look at the vote totals and may be stampeded.

He explained that many amendments are very complex:

> There may be only three or four or five people in the House—on our side of the House—who understand what some of these amendments really do, and if they don't have a way to reach the whole Democratic caucus, we're in trouble. On the ______ bill, every time we didn't have one we lost.

A task force chairman explained:

> You know these members are coming in through the door and they have essentially 7 to 2 minutes or even a minute to vote on an amendment. They're running in and asking, "What is it?" and you can't explain a four-page amendment, changing aggregates and everything else and subfunctions. So you say, "This is the Grassley of Iowa amendment, vote no," and hand them the sheet. It points out as succinctly as possible what's in it—that it adds $380 million to the deficit—that it cuts out all programs for babies.

If the bill is on the floor for more than one day—an increasingly frequent occurrence—the task force will meet between sessions. Committee staff will brief the members on amendments expected, arguments to be used will be discussed, voting records to that point will be analyzed. The task force may decide that on a given amendment "20 members are targets and everyone will work on them."

Sometimes the current situation will suggest new tactics. Republicans could not agree upon one substitute to the first budget resolution in 1979; consequently, two alternative plans were introduced and brought to a vote. This allowed the leadership and the task force to persuade those Democrats who believed they had to go on record as favoring budget cuts to vote for one or the other, but not both, and to make sure such members divided their votes between the two alternatives evenly so that both would fail. In fact, 33 Democrats did so vote and neither Republican alternative passed.

In addition to orchestrating persuasion efforts, the task force chairman sometimes provides advice on floor strategy. Whether to finish a bill even though it means staying very late or to pull a bill off the floor after one or two amendments have been lost can make a major difference. On important bills, the floor manager and the leadership make such decisions; but the task force chairman can provide another judgment. According to a staffer:

> On ______, we had a bad problem and the task force chairman insisted that we leave the floor and saved us. We went back and we had a day or a day and a half to regroup and, in this case, it was the presence of the task force that pushed us off the floor. The committee sort of wanted to stay. It was the task force chairman who made the difference in getting off the floor. He said, "Hey, look, I know you'd like to stay and finish the bill. Well, I happen to think that if we stay here another five minutes, we're not going to have a bill to manage any more." It wasn't quite that blunt, but it was along these lines, and that got us off the floor. . . . A task force chairman provides another judgment as well as that of the committee members who are often so close they don't see it too well, whereas the task force chairman, though he's pretty close himself, sometimes senses the mood a little better and can determine a little better whether to quit or stay.

By performing tasks the formal leadership group is too small to carry out alone, the task force does increase the probability of floor success. The task force can explain the provisions of a complicated bill on a member-to-member basis; it can get a detailed and precise count of Democrats' voting intentions and engage in face-to-face persuasion efforts with a large proportion of the Democratic membership. It has the organization and the numbers to respond quickly to the unexpected floor developments.

Evaluation of Task Force Success

One of the leadership's objectives in setting up a task force is passing the legislation in question. According to that criterion, all task force efforts have been successful. Because the bills on which task forces worked were highly controversial, winning was seldom easy and victory not always clear-cut. In several cases, success came after an initial defeat and, in some, compromise at the post-committee stage was necessary. In at least one case, the bill reported by the committee was not at all to the Speaker's liking. Nevertheless, all the legislation on which task forces worked was passed without the addition of crippling amendments at the floor stage. The leaders believe the task force device has proven itself as a coalition-building strategy well suited to the post-reform House.

From the beginning, the Speaker saw the task force strategy as having a benefit beyond that of passing a particular bill. "Give me a frustrated guy," the Speaker says, "and I'll give him a job to do and he becomes a 'Tip O'Neill man' " (*New York Times,* June 4, 1979). Rather than attemption to dampen the high rate of rank-and-file participation—an enterprise almost certainly doomed to failure—the leadership seeks to direct it into productive channels. In the process of participating in leadership-directed efforts, it is hoped that Democratic members, particularly the junior ones, will learn the need for "followership" and for some joint action under the aegis of the party.

Participants believe that task forces have been useful in socializing junior members. Working on a task force gives them an understanding of the problems of the leadership and, in some cases, results in an identification with the leadership. One participant explained:

> It's helped the leadership to get to know the new members and the new members to get to know the leadership. And it's certainly helped the new members who I've spoken to—my personal friends—to understand the need for leadership and followership. I think the guys who have served on task forces know a lot more about the need for a party structure with some loyalty than those who haven't.

According to another participant:

> The task force members, particularly the chairmen, got an awareness of the problems of leadership and a feeling of meaningful participation that paid dividends on later legislation, as they felt some obligation to cooperate with other task forces. Many of the task force members, like ______, became emotionally involved with the leadership.

As the socializing effect became clearer, the task force membership selection process changed. There is now a conscious effort to include Democrats specifically for the effect membership may have on their attitudes towards intraparty cooperation. As one observer put it:

> Initially—though not necessarily totally consciously—we chose those people who might be most amenable to working in the same philosophical mold as the man who appointed them, the chairman, and the Speaker, and the leadership as a whole. Though not totally—I mean that's not the only reason you choose somebody. But I think what happened was that we realized after a couple of task forces had gone by that we were doing that to an extent greater than we should, and have since then changed the nature of the invitation process a little bit to try and broaden it and bring in people who we're not that used to working with, so to speak. And there's been some discussion about that, and there is a conscious effort currently being made to broaden it, to reach into places we haven't reached into before. You can't always do it because the places you haven't reached into before may be philosophically totally opposed to your bill. But we try, and we're trying pretty hard.

Increased awareness of the socialization benefit of task force membership seems by the 96th Congress to have led to another form of reaching out in making up task force membership lists. Every one of the five new Democratic members of the Ways and Means Committee was included in the debt-limit task force in the spring of 1979. Of the eight new Budget Committee Democrats, six served on the budget-resolution task force; deep policy disagreements precluded the inclusion of the remaining two. This appears to represent a conscious attempt to draw new committee members into the leadership's orbit, to accustom them to working with the leadership on major bills within their committee's jurisdiction.

The transference of techniques learned working on a task force to other situations is still another benefit. A participant explained:

> Members who had previously served on task forces undertook to do on the part of their committee that which previously they had done on task forces. In other words, they got some new tools, they saw that they worked, and now they're carrying them back into their work for their committee. I think that's fantastic. You see members of the committee taking sheets of paper [explaining amendments] to the doors. It seems to be becoming more the way you handle things, a more routine kind of thing and I think that's just great.

Conclusion

At the core of successful leadership is satisfying the expectations of followers. Consequently, appropriate strategies change when members' expectations and other aspects of the environment which bear upon the satisfaction of those expectations change. In a House dominated by powerful committee chairmen, one in which junior members did not expect to play a significant legislative role and even senior members largely restricted their activity to their own committees' legislation, a strategy of inclusion was unnecessary and inappropriate. The post-reform House, in which rules allow and member expectations dictate wide participation, requires such a strategy. The use of task forces increases the probability of passing major legislation on the now highly unpredictable House floor. By satisfying members' expectations of significant participation in the process, it contributes to "keeping peace in the family." The leaders believe that service on task forces, in addition, teaches members the value of cooperative behavior.

As the leaders themselves recognize, their success depends not only upon what they do but also upon forces in the environment outside their control. Their ability to satisfy members' expectations about legislative results depends upon some core consensus within the Democratic party. No member expects the leadership to tailor its legislative priority list to his or her individual preferences; all realize the leadership must be responsive to the diversity of interests within the House Democratic membership and to significant actors outside the chamber. The legislative record as a whole must, however, be reasonably satisfactory to a substantial majority of the party membership. The failure to produce legislation that satisfies members' reelection needs and their policy goals cannot be fully compensated for by internal rewards. If, as now seems possible, the Democratic consensus on core elements of social welfare and economic policy totally disintegrates, the leaders will be caught in a conundrum. To the extent that they are successful at building winning coalitions in response to the demands of some set of members, they may so dissatisfy other members that their very success will be extremely expensive in terms of "keeping peace in the family." Each coalition-building effort may exacerbate intraparty conflicts, discourage cooperative behavior, and consequently, make future coalition-building success less likely. The continued adequacy of the strategy of inclusion and its success at fostering norms of intraparty cooperation, thus, depend upon the reestablishment of some consensus within the Democratic party.

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Institutional Context and Leadership Style: The House from Cannon to Rayburn

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This article deals with the transition in House leadership from Cannon to Rayburn. The transition involved moving from a hierarchical pattern of leadership to a bargaining pattern. In accounting for this transition, we argue that it is the institutional context of the House that determines leadership power and style. Moreover, we argue that there is no straightforward relationship between leadership style and effectiveness; rather, style and effectiveness are contingent or situational. We conclude that the impact of institutional context on leadership behavior is itself primarily determined by party strength. When party strength is high, power is concentrated and leaders are task- or goal-oriented, whereas when party strength is low, power is dispersed and leaders will be oriented to bargaining and maintaining relationships.

Leadership is an aspect of social life which has been extensively studied in a variety of institutional or organizational settings (Miner, 1980). Yet it remains a topic in which our intellectual grasp falls far short of our pragmatic sense of the impacts leaders have on organizational operations and performance.

This is as true, if not more true, of Congress than of other organizations. Here too analysts are perplexed by the difficulties of conceptualizing key variables, treating highly transient and idiosyncratic personal factors, and identifying relationships amidst a maze of interactive effects. Moreover, the task is rendered even more complex by the highly politicized character of the Congress as compared with most of the organizational contexts in which leadership has been studied.

This is not to say that knowledge and understanding of congressional leadership have remained static. Peabody (1976), Jones (1968), Ripley (1967), Polsby (1969), Manley (1969), Hinckley (1970), and Nelson (1977) have all done instructive and insightful work. Nonetheless, our grip on the topic is as yet not firm; we continue to lack a developed sense of what we should be looking at and how to proceed.

The purpose of this article is to aid in remedying this deficiency. It is premised on two key assumptions. First, the study of leadership requires comparative evidence regarding both behavior and contexts. Hence our use of history as a laboratory and our choice of Cannon and Rayburn as focal points of analysis. Second, the study of leadership requires abstract or analytical concepts to aid in formulating and testing important relationships. Hence our historical analysis relies on several broad concepts and relationships, drawn both from organization theory and from recent work on the operation of the Congress.

In sum, though this article deals with the transition from Cannon to Rayburn, its main objective is not to fill in the historical record. Its primary goals are rather to bring evidence and analysis to bear to improve our understanding of the key determinants and underlying dynamics of congressional leadership, and to suggest a set of propositions or hypotheses that can serve as a basis for more focused and elaborate forms of investigation and theory building.

The House Under Czar Rule

The legacy the House of the nineteenth century left to that of the twentieth was a set of rules which placed the majority firmly in control of the House and centralized power in the hands of the Speaker as the agent of this majority. It was this legacy the House rejected when it revolted against the Speaker in 1910. In so doing it not only stripped the Speaker of many of his important powers, but also paved the way for a metamorphosis in the nature of the House as a political institution.

The Speaker and the House. It was with good reason that Speakers of the House in the years between 1890 and 1910 were often referred to as czars (Galloway, 1961, pp. 134-36). The Speaker appointed the committees. He served as the chairman of and had unchallengeable control over the Rules Committee (Brown, 1922, pp. 87-90). He had great, though not unlimited, discretion over the recognition of members desiring to call business off the calendars, to make motions, or to address the House, and absolute discretion over the recognition of motions for unanimous consent and suspension of the rules (Chiu, 1928, pp. 175-97).

These prerogatives gave the Speaker great power to control outcomes in the House. At the committee stage, those who had received prized assignments from the Speaker naturally felt a sense of gratitude and obligation to him. Those who desired a change in assignment knew full well that their chances of advancement depended on the good graces of the Speaker. Conversely, since in this age seniority was far from as sacrosanct as it is today, members were also aware that to alienate the Speaker was to risk loss of a chairmanship, an assignment, or rank on a committee (Abram and Cooper, 1968; Polsby et al., 1969).

Nor was the appointment power the Speaker's only source of leverage in controlling outcomes at the committee stage. Members of any particular committee were also disposed to cooperate with the Speaker because of the vast array of rewards and sanctions his position in the House bestowed on him. For example, the Speaker could provide access to the floor by granting a rule or recognizing a motion to suspend the rules; he could lend invaluable assistance in getting a project included in a bill or in getting a bill out of committee. Moreover, if all the rewards and sanctions at the disposal of the Speaker still proved to be insufficient, there was yet another factor that discouraged opposition at the committee stage. The plain fact was that to oppose the Speaker would in all probability be fruitless. If a committee refused to report a bill the Speaker wanted reported, the Speaker could pry the bill out of committee either through use of the Rules Committee or suspension of the rules. Similarly, if a committee reported a bill the Speaker opposed, the bill had little chance of reaching the floor. The power of the Speaker was such that he could obstruct the consideration of any bill he did not want considered. Given the various and potent types of leverage the Speaker possessed, it is not surprising that in this period committee chairmen took their cue from the Speaker regarding which bills they would report and that Speakers referred to the committee chairmen as their "cabinet" (Busby, 1927, p. 219).

As for action on the floor, here too the Speaker's prerogatives under the rules gave him great power. A number of factors combined to give him control over the agenda of the House. Through use of the Rules Committee and other privileged committees, he could interrupt the regular order of business either to give priority to a bill he wanted considered or to block a bill he opposed.[1] He could use unanimous consent and suspension of the rules to give access to the floor to bills he favored and could deny the use of these procedures to bills he opposed. In addition, his discretion in the recognition of motions calling bills up for consideration was a source of leverage.

The Speaker's ability to control the agenda, however, stemmed not merely from his powers of repression, but also from the necessity of relying upon him if the House was to reach the bills it wanted to consider. The volume of legislation before the House made it exceedingly cumbersome to follow the involved order of business set forth in the rules. As a result, the House did not insist on following its regular order. Indeed, the points in the order where committee members could call business off the calendars were usually not reached. Instead, the House relied on the Speaker to bring bills up for its consideration and to determine the time of consideration through the use of privileged reports and special procedures, such as unanimous consent. In short, then, both because of the powers of the Speaker over the agenda and the unwieldiness of proceeding according to the regular order, the House gave the Speaker even more power over the agenda than his power under the rules bestowed on him.[2]

A second aspect of the Speaker's power over floor decisions concerned his ability to control considerations on the floor. Here again, his command of the Rules Committee and his power as presiding officer gave him considerable leverage over floor debate and dilatory tactics. In addition, many of the same rewards and sanctions at the Speaker's disposal for controlling committee decisions could also be used to control floor decisions. Especially for members of the majority party, to oppose the Speaker was to risk the loss of his assistance in matters of vital importance to one's constituency and therefore also to impair one's chances of reelection. Moreover, through bestowing favors over a number of years, the Speaker could build up a substantial fund of credits, credits which could then be expended as needed to secure the cooperation of members in his debt. Thus, the ability of the Speaker to control decisions on the floor and in committee stemmed not only from the immediate impacts the exercise of his formal powers involved, but also from their long-run dividends.

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[1] Aside from the Rules Committee, nine other committees were privileged to report at any time on certain matters. In addition, the eight committees charged with general appropriation bills also were privileged to report at any time on such bills. *Hinds' Precedents,* Vol. 4, Sec. 4621. It should also be remembered that the unanimous consent procedure at this time consisted simply of motions on the floor to that effect. The rule providing for a Consent Calendar was not adopted until 1909 (Chiu, 1928, pp. 179-88).

[2] The total control the Speaker exercised over the consideration of minor legislation thus resulted from the combined impact of the lack of a regular order of business and the need to secure recognition by the Speaker to use either unanimous consent or suspension to bring constituency bills to the floor (Chiu, 1928, pp. 228-30).

The Speaker as Party Chief. To complement his prerogatives under the rules, the Speaker possessed another source of power that was equally significant. In placing a potent array of rewards and sanctions at his disposal, the rules did of course provide him with considerable leverage. However, the Speaker's ability to command majority support in committee and on the floor was materially aided by another factor: party discipline.

In an age when party regularity is far from an overriding consideration, it is difficult to appreciate how important party was in the House at the turn of the century. In this period the great majority of members in both parties subscribed to the doctrines of party government. Representative government was seen to depend on the existence of a responsible majority which had the power to rule and which, as a result, could be held accountable for performance (Jones, 1968). Only under such conditions, it was believed, could the people effect their wishes. The individual representative was thought to be elected on the basis of a party's platform and was therefore regarded to have an obligation to support party positions, even against personal convictions or desires.

The fact that members in this age thought and spoke in terms of the doctrines of party government had more than rhetorical importance. Party government served as the main justification for vesting great power in the Speaker and permitting him to play the role of Czar. Though the Democrats never fully accepted the proposition and ended by rejecting it, a cardinal tenet of Republican faith was that rule by a responsible majority party required centralizing organizational power in the Speaker.

The Speaker's position as head of his party thus also provided him with an important source of leverage. It is true, of course, that even in this period many issues were not treated as party issues. Nonetheless, most important issues were regarded as matters on which the party as a whole should stand together. In such a context the Speaker derived considerable power from his position as party chief. Initiative in the definition of party policies belonged to him. Moreover, if he could not win the support of all elements in the party, he had at his disposal a powerful mechanism for enforcing adherence to his wishes—the caucus. Through a binding vote in the caucus, he could oblige the opposition to support his policy positions out of party loyalty (Brown, 1922, pp. 92-93, 100, 161-62; and Wilson, 1961, p. 96). In short, the Speaker could rule the House through the force of party discipline. As long as the bonds of party held taut, he had only to command the support of a majority of his party to command a majority in the House.

We may conclude, then, that the House of Reed and Cannon contained a highly centralized power structure with control resting essentially in the hands of the Speaker. The key to the Speaker's power lay not simply in his prerogatives under rules, nor in his position as party chief, but rather in the manner in which these two sources of leverage reinforced each other (Cooper, 1970). The existence of a stable party majority insured the Speaker's ability to implement his formal powers and gave him a degree of maneuverability and control that the rules alone could not give him. Similarly, the rewards and sanctions the rules placed in the Speaker's hands gave party regularity a degree of priority it would not have possessed if it had rested merely on the extent of agreement among party members or their devotion to the doctrines of party government. Speakers could therefore quite appropriately refer to committee chairmen as their "cabinet." During this period the committee and party systems were blended to an extremely high degree. The Speaker was both the party leader and the chairman of the Rules Committee. The majority leader was the chairman of the Ways and Means or Appropriations Committee and, with the start of the whip system in 1897, the whip was chairman of Judiciary or a top member of Ways and Means. Unlike the contemporary House where party leaders and committee chairs are separate, committee and party leaders were one and the same. Tensions between the two systems were accordingly greatly reduced. Whereas in the contemporary House committee chairs often have low party support scores, such was not the case at the turn of the century. It was thus not a mere figure of speech to refer to committee chairs as a "cabinet." Both structurally and behaviorally, committee and party leaders were a cabinet (Brady, 1973).

The Bases of Czar Rule. We have argued that the interaction of the Speaker's formal powers and the strength of party resulted in a centralized form of leadership—Czar rule. This can be shown empirically. Data exist which strongly buttress the

Table 1. Czar Rule and Levels of Party Voting in the House (1881-1921)

Congress	Year	Percentage Party Votes	Majority Party	Centralized Leadership
47	1881	16.6	Republican	No
48	1883	7.0	Democratic	No
49	1885	15.5	Democratic	No
50	1887	8.7	Democratic	No
51	1889	42.5	Republican	Yes
52	1891	4.2	Democratic	No
53	1893	6.1	Democratic	No
54	1895	24.8	Republican	Yes
55	1897	50.2	Republican	Yes
56	1899	49.8	Republican	Yes
57	1901	38.9	Republican	Yes
58	1903	64.4	Republican	Yes
59	1905	34.6	Republican	Yes
60	1907	26.3	Republican	Yes
61	1909	29.4	Republican	Yes/No
62	1911	23.0	Democratic	No
63	1913	19.9	Democratic	No
64	1915	21.7	Democratic	No
65	1917	9.4	Democratic	No
66	1919	14.9	Republican	No

Source: David Brady and Phillip Althoff, "Party Voting in the U.S. House of Representatives, 1890-1910: Elements of a Responsible Party System," *Journal of Politics* 36 (1974): 753-75.

argument we have made deductively on the basis of the historical record.

First, if party strength functioned as a key ingredient of Czar rule, then levels of party voting should be markedly higher in congresses with centralized leadership. Table 1 presents data on party votes (90 percent of one party versus 90 percent of the other) for congresses from 1881-1921. It thus includes "Czar rule" congresses in which the Speaker possessed the formal powers described above and a ready ability to use the caucus, and congresses in which one or more of these sources of leverage was absent.

Table 1 shows a strong connection between centralized leadership power and levels of party voting. In the period from 1881 to 1899 party voting scores attained levels of 25 percent or more only in the three congresses in which centralized leadership power existed. In the period from 1899 to 1921 party voting did not drop below 25 percent until after the 1910 revolt against the Speaker, and then fell to below 10 percent in 1917 for the first time in a quarter-century. To further substantiate our argument, we ran a point bi-serial analysis of the data. This statistic is used when the data are dichotomous and is appropriate for Table 1, given a distinction between centralized and non-centralized leadership power. The point bi-serial for this data set was a striking .89, demonstrating the degree to which levels of party voting can be seen as associated with concentrated leadership power.

Second, if an interactive relationship exists between concentrated formal power and party strength, then party strength must have its own sources of determination and impact. Indeed, in our view the causal impact of party strength on the distribution of power in the House is of primary importance. For the Speaker to have the power involved in Czar rule, a majority of the House members had to agree to bestow such power. Since the House is organized on the basis of party and since during this period the Republicans were usually in the majority, it was their potential for group cohesion and loyalty that established the conditions for centralized leadership. In short, the vehicle through which centralized leadership developed was the congressional Republican party (Brown, 1922, pp. 71-126). The rationale underlying this development was that without party government the industrial gains of the late nineteenth century would have been negated by congressional Democrats.

However, to sustain the role and significance we have accorded party strength, we must be able to identify and demonstrate independent sources of determination. In this respect it may be noted that the development of strong party systems in Europe and Britain is associated with the rise of leftist-socialist parties and that in the United States those states where the parties represent polarized constituencies have high levels of party voting. Our argument is therefore that the fundamental bases of party strength at the turn of the century, as in all periods of our history, are largely external, that party strength is rooted in polarized constituency configurations.

In order to ascertain the constituency bases of the congressional parties as well as the differences between them, we calculated the degree to which each congressional party represented agricultural as opposed to industrial districts. For example, in the 55th House (1897-1899), 69 percent of the Democrats and 26 percent of the Republicans represented agricultural districts, that is, districts where the ratio of farms to industrial workers was at least three to one. Thus the difference between the parties was 43 percentage points. This differential was computed for the 47th through the 66th Houses (1881-1921) and serves as a measure of electoral polarization. The specific hypothesis is that there should be a strong relationship between polarization and party voting. Table 2 confirms the hypothesis. When polarization was high, so too was party voting. Conversely, in Houses where the differential was less than 20, that is, where the parties were less polarized, the proportion of party votes did not rise above 20 percent and dropped to as low as 4.2 percent. However, perhaps the best overall statistic is Pearson's r, which is .81 for the two variables presented in Table 2.

Table 2. Polarization of Parties and Party Voting in the House (1887-1921)

Congress	Year	Differential	Party Votes
47	1881	36	16.6
48	1883	15	7.0
49	1885	25	15.5
50	1887	24	8.7
51	1889	41	42.5
52	1891	19	4.2
53	1893	22	6.1
54	1895	36	24.8
55	1897	43	50.2
56	1899	33	49.8
57	1901	35	38.9
58	1903	39	64.4
59	1905	41	34.6
60	1907	36	26.3
61	1909	31	29.4
62	1911	24	23.0
63	1913	12	19.9
64	1915	11	21.7
65	1917	14	9.4
66	1919	18	14.9

Source: David Brady and Phillip Althoff, "Party Voting in the U.S. House of Representatives, 1890-1910: Elements of a Responsible Party System," *Journal of Politics* 36 (1974): 753-75.

The data also show that during the period from the realignment of 1894-96 to approximately the election of Woodrow Wilson (the 54th through the 61st House), the parties remained polarized and levels of party voting remained high. On the other hand, during the "period of no decision" (47th through the 53rd Houses), the degree of polarization fluctuated, and levels of party voting varied accordingly. Similarly, after 1908 the congressional parties became less polarized as the Democratic party became more competitive in industrial districts, and party voting in the House again declined.

In sum, then, it is critical to note the correspondence between a polarized electoral system and a highly centralized leadership structure. Though the Speaker's formal powers reinforced party strength, the polarized electoral bases of the party system provided an indispensable platform for Czar rule. Thus, when electoral polarization began to decline, the centralized internal structure also began to come apart (Brady et al., 1979).

The House from Cannon to Rayburn

Despite its power, the system of Czar rule could not maintain itself. It proved to be too rigid a system to accommodate the factional tendencies in the party system. During the early years of the twentieth century, economic and social ferment in the Midwest and West brought to Congress a group of young Republicans passionately devoted to enacting a whole series of reform measures. Cannon used his power as Speaker and party chief to contain and frustrate the desires of these members. In so doing, he soon aroused their enmity not merely for his policies but also for the whole system of power then prevalent in the House (Jones, 1968).

The Revolt against the Speaker. Though the number of Insurgent Republicans in the House was never large, by 1909 their strength in combination with the Democrats was sufficient to bring the revolt to a successful conclusion. The first step came in 1909 with the establishment of a Consent Calendar and a call of the committees every Wednesday to take up business on the House or Union Calendars. At this time more sweeping change was prevented by the defection of a group of conservative Democrats (Hechler, 1940, pp. 42-63). The next year, however, the Insurgent Republican-Democratic coalition gained a decisive victory. On March 19, 1910, after a dramatic two-day fight, the House passed a resolution removing the Speaker from the Rules Committee, enlarging its membership, and providing for election of the committee by the House. This victory was followed two months later by the passage of a resolution which established a procedure through which individual members could initiate the discharge of bills from committees (Brown, 1922, pp. 143-88). Finally, in 1911 the last major objective of the opponents of Czar rule was achieved. The House, now under Democratic control, amended its rules to provide for the election of all standing committees and their chairmen (Hasbrouck, 1927, p. 11).

The immediate results of the revolt against the Speaker did not greatly impair the ability of the party leadership to lead the House on behalf of the party majority. In acting to weaken the Speaker, the Democrats had no intention of weakening the ability of the party majority to pass its program. Most Democrats believed as strongly in party government as most Republicans. Their objection was not to party government and party responsibility but to domination of the majority party and the House by the Speaker. Thus, when the Democrats gained control of the House in 1911, they set up an effective system of rule through the majority party. On the one hand, they made extensive use of the caucus and binding votes in caucus (Haines, 1915, pp. 53-110). On the other hand, they centralized power in the party by making the chairman of the Committee on Ways and Means, Oscar Underwood, both floor leader and chairman of the committee on committees.[3] Under Underwood's leadership, the Democrats controlled the House as tightly as the Republicans had under Cannon. Indeed, it is fair to say that the Insurgent Republicans were no happier in the new "reformed" Democratic House than they had been in the old "tyrannical" Republican one. They had no greater liking for "King Caucus" than for "Czar rule."

The long-run results of the revolt, however, were quite different. If Czar rule was unable to maintain itself in the face of centrifugal pressures in the party system, caucus rule was even less fitted to do so. In the absence of the buttress the formal powers of the Speaker provided for party cohesion, increases in factional discord within the party alignments easily asserted themselves and led both to a disintegration of party control mechanisms and to a dispersion of power within the House (Cooper, 1961, 1970).

The disintegration of party control mechanisms was gradual but extensive. The caucus was the first to go. Once the Democrats achieved the major items in their domestic program, the power of the caucus began to wane. From 1916 on, the divisions within the parties made it difficult to rely on the caucus and usage quickly declined (Luce, 1922, p. 513). This is consistent with the data presented in Table 1 that shows party voting at less than 15 percent in the 1917-1921 period—a 22-year low. There were small upsurges in activity in the early 1920s and early 1930s during the initial years of party turnover in the presidency. However, its use for policy purposes soon became rare in the 1920s and simply disappeared in the late 1930s. Thus by the end of the 1930s the caucus was virtually moribund as a mechanism for determining party policy (Kefauver, 1947, pp. 102-03).

When the Republicans regained control of the House in 1919, they set up a steering committee and began to rely on it rather than the caucus (Brown, 1922, pp. 195-224; Chiu,, 1928, pp. 329-34). Though this committee from the first was less of a control device than the caucus and more of a coordinating and planning mechanism, during the early 1920s it did serve to augment the leadership's power to direct its partisans. However, the same tendencies toward factionalism and bloc voting that reduced the caucus to marginal significance had a similar effect on the steering committee. By the late 1920s the party leadership had come to see the steering committee as a hindrance to their maneuverability and effectiveness. As a result, they abandoned the mechanism and began to rely instead on informal meetings among themselves, i.e., on an informal board of strategy composed of the Speaker, the floor leader, and a few trusted lieutenants (Chiu, 1928, pp. 334-36). The situation did not change when the Democrats took control of the House in 1931. Though they too established a steering committee, their leadership operated in much the same fashion as the Republican leadership had in the late 1920s (Galloway, 1961, p. 145). In short, then, by the late 1920s reliance on party control mechanisms to coordinate action and enforce cohesion had largely passed from the scene. Instead, the majority party was reduced to operating primarily through a small coterie of men, gathered around the Speaker, who met to plan strategy and whose power of direction was much less than that of the caucus or even the steering committee in their heyday.

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[3] In changing the rules to provide for election of the standing committees by the House, the Democrats took care to maintain centralized party control. Thus, they blocked the alternative preferred by the Insurgent Republicans, which would have provided for party committees on committees elected by geographic regions. Instead, the Democrats adopted a rule that only provided for election by the House and then made the Democratic members of Ways and Means, led by Underwood, their committee on committees (Brown, 1922, pp. 172-77, and 62 *Congressional Record* 1, pp. 63-64).

Nor were the caucus and the steering committee the only party control mechanisms to lose power and effectiveness in the period after 1916. The power of party mechanisms to control committee personnel also declined. Republican Speakers from 1890 to 1910 respected seniority, but they were quite prepared to violate it in the interests of party policy. The same is true of Underwood. By the 1920s the situation was substantially different. The decline of the caucus and, to a lesser extent, of the steering committee enhanced the power and independence of party factions. Their sheer willingness to stand together and cooperate with the leadership became more important than ever before. In addition, as the power and independence of party factions increased, the appointment mechanisms became more decentralized. Thus, by 1919 the Republicans had taken the power of appointment from the leader of the party and had vested it in a committee on committees, composed of nearly 40 members. Similarly, after 1923 the Democrats no longer combined the posts of floor leader and chairman of the committee on committees. In such a context seniority was transformed from an important consideration to a sovereign principle. It alone provided a standard in terms of which decentralized appointment mechanisms could distribute key committee positions among party factions without provoking disputes that would weaken the party. As a result, in contrast to earlier eras, departures from seniority were rare in the 1920s and even rarer thereafter (Abram and Cooper, 1968; Polsby et al., 1969).

From Hierarchy to Bargaining. Given the reductions in the formal powers of the Speaker between 1909 and 1911, the disintegration of party control mechanisms after 1916 produced a dispersion of power in the House. If in Cannon's day the Speaker's prerogatives as Speaker and as party chief combined to centralize power in the House, now the reduction in the formal powers of the Speaker and the disintegration of party control mechanisms combined to decentralize power in the House.

On the one hand, the rewards and sanctions which the rules placed in the hands of party leaders were reduced. The party leadership no longer had absolute control over committee appointment, the Rules Committee, or the consideration of minor business. On the other hand, the ability of party leaders to consolidate and maintain support in their own ranks was also reduced. If it is true that factionalism in the party system led to the decline of party control mechanisms, it is also true that the decline of these mechanisms had the further effect of allowing party factionalism greater expression. The result of these developments was to heighten the power and independence of the individual member and of key organizational units in the House. Denied the power they possessed over the individual member under Czar rule or caucus rule, party leaders began to function less as the commanders of a stable party majority and more as brokers trying to assemble particular majorities behind particular bills. Denied the power they possessed over the organizational structure under Czar rule or caucus rule, party leaders began to function less as directors of the organizational units and more as bargainers for their support.

These tendencies intensified as time passed. During the 1920s the breakdown of the steering committee and the rise of seniority to predominance cast party leaders more firmly in the roles of brokers and bargainers than had been the case at the start of the decade (Chiu, 1928, pp. 315-36 and Hasbrouck, 1927, pp. 48-50). Similarly, events during the 1930s confirmed and strengthened these roles. If the level of party cohesion during the 1920s was not high enough to permit reliance on party control mechanisms, it was still of such proportions that in general the holders of key organizational positions were loyal to the leadership and willing to cooperate with it. Nor, despite the increases in factionalism and bloc voting, did party leaders during the 1920s confront any stable and comprehensive basis of division among their fellow partisans, any extensive and consistent split across a whole range of issues. By the late 1930s, however, the situation had changed in both these regards.

After a brief increase in party voting during the initial years of the New Deal, party strength again began to decline in a steady and substantial fashion (Sinclair, 1978). Moreover, this decline gave birth to a new and distinctive feature, the Conservative Coalition (Brady and Bullock, 1980; Manley, 1977). Table 3 provides supportive data on both trends.

Thus, as the 1930s came to an end, party politics in the House began to display characteristics and configurations that were to become entrenched in the 1940s and to endure for several decades. These changes, however, made the task of the majority party leadership more, not less arduous. First, party divisions in the majority party now assumed a pronounced bifurcated form. In seeking to build majorities from issue to issue, the leadership accordingly was frequently threatened with the loss of support of a substantial portion of the southern wing of the party, a wing that from the late 1930s to late 1950s was roughly equal in size to the northern wing of the party (Cooper and Bombardier, 1968). Second, the divisions within the majority party now began to be translated into the organizational structure in a manner that far exceeded previous experience. The party leader-

Table 3. The Decline of Party Voting in the House and the Rise of the Conservative Coalition (1909-1953)

Congress	Year	Percent Party Votes	Percent Coalition Activity	Percent Coalition Victories
61	1909	29.4	–	–
62	1911	23.0	–	–
63	1913	19.9	–	–
64	1915	21.7	–	–
65	1917	9.4	–	–
66	1919	14.9	–	–
67	1921	35.2	–	–
68	1923	13.4	–	–
69	1925	5.3	3.5	63.5
70	1927	5.6	1.4	100.0
71	1929	13.6	5.8	80.0
72	1931	13.8	4.9	62.0
73	1933	18.9	2.1	48.0
74	1935	14.2	4.3	56.0
75	1937	11.8	7.6	67.0
76	1939	17.6	9.3	95.0
77	1941	10.5	12.5	92.0
78	1943	9.6	21.8	96.0
79	1945	12.1	22.1	88.0
80	1947	12.7	19.6	100.0
81	1949	6.5	16.4	83.0
82	1951	4.9	24.9	86.0

Sources: David Brady, Joseph Cooper and Patricia Hurley, "The Decline of Party Voting in the U.S. House of Representatives," *Legislative Studies Quarterly* 4 (1979): 381-407; David Brady and Charles Bullock, "Is There a Conservative Coalition in the House?" *Journal of Politics* 42 (1980): 549-59.

ship's ability to use the machinery of the House to suit its own purposes accordingly declined. It began to encounter difficulty securing the support of particular committees and committee chairmen much more frequently. This was especially true of the one committee in the House on which the leadership was most dependent and which historically had always been regarded as falling within the province of the leadership—the Rules Committee. For the first time in history the leadership found itself confronted with a Rules Committee that regarded itself and acted as an independent agent, rather than as an arm of the leadership (Galloway, 1961, pp. 145-48; Jones, 1968).

These developments further weakened the power and position of the leadership and in so doing further enhanced the independence of individual members and organizational units. Moreover, the impact was long-lasting, not transitory. A divided majority party was less amenable to leadership direction and control than an incohesive one. From the late 1930s on, the leadership was forced to place even more reliance on brokerage and bargaining than had been necessary in the early 1930s or 1920s (Herring, 1940, pp. 21-45).

The Rayburn House

The period from 1910 to 1940 may therefore be seen as a period of transition in the character of the House as a political institution. By 1940, the year Sam Rayburn assumed the Speakership, a new and distinctive type of House had emerged. It was a House that was destined to endure in most of its essential features until the reform of the Rules Committee in the early 1960s and in many of its essential features until the reemergence of the caucus in the late 1960s (Cooper, 1970; Brady et al., 1979).

The House under Decentralized Rule. The Rayburn House was a far different body from the House of Cannon or Reed. Centralization of power and hierarchical control had given way to a diffusion of power and bargaining.

On the one hand, the majority party leadership could no longer command the organizational units due to the breakdown of party control mechanisms and the elimination of the Speaker's prerogatives over appointment and the Rules Committee. Rather, it had to seek to win their support and do so in a context in which divisions in the majority party had become so pronounced that they had begun to appear at key vantage points in the organizational structure. On the other hand, the majority party leadership could no longer command overwhelming support from the ranks of its partisans on the floor due both to the decline in party strength and the decline in the fund of rewards and sanctions at its disposal. Rather, it had to seek to build majorities from issue to issue and do so in a context in which a deep split existed in the ranks of the majority party and distaste for party discipline was intense and pervasive. Political scientists writing about the House in the 1940s and 1950s accordingly emphasized themes quite different from those emphasized in the initial decades of this century: the primacy and amount of catering to constituency, not party loyalty or discipline; the dispersed and kaleidoscopic character of power in the House, not the authority and responsibilities of party leaders; the role of committee chairmen as autonomous and autocratic chieftains, not their operation as loyal party lieutenants (Young, 1943; Gross, 1953).

However, the fact that power became decentralized in the House does not mean that significant centers of power did not continue to exist. What occurred was a wider dispersal of power, not its fractionalization.

First, the party leadership retained substantial ability to influence and even control outcomes in the House. If party voting decreased, the party bond remained important both because of the degree of agreement still present and because of the interest most members had in establishing some kind of party record. Thus, though the leadership could no longer rule the House on the basis of votes drawn from its own party, it could still usually count on a large and stable reservoir of support from its fellow partisans (Mayhew, 1966). In addition, party leaders continued to derive leverage from other sources. The formal powers remaining to the Speaker aided their ability to control access to the floor and proceedings on the floor. The influence party leaders maintained over the party committee on committees enabled them to alter the political complexion of particular committees through the screening of new appointments. The power party leaders retained, due to their positions in the House and in the party, to dispense favors and build up credits augmented their capacity to secure the cooperation of ordinary members and holders of organizational positions (Ripley, 1967). Finally, the leadership could rely on the president's influence to win the support of reluctant partisans both in committee and on the floor.

Second, committees and committee chairmen emerged as rival power centers of great importance. In a context in which House rules gave the committees immense power over the handling of legislation within their jurisdictions and committee rules and practice gave their chairmen immense power within their committees, the decline in leadership authority and power redounded to the advantage of the committees and their chairmen. Typically, committee opposition to legislation sealed its fate, even when favored by the leadership. Conversely, committees that operated in a unified fashion were accorded great deference on the floor and had high levels of success (Fenno, 1962). Party leaders thus could not treat committees merely as instruments of their will nor chairmen simply as loyal lieutenants. Rather, they had to function largely as petitioners of committee support and floor managers of committee legislation.

In the Rayburn House the committees accordingly reemerged as the feudal baronies they had been in the decades immediately preceding Czar rule. And, indeed, to a significant degree the story of the Rayburn House is a story of conflict among northern majorities in the Democratic party, the majority party leadership, and southern-dominated committees in which northern pressure for action was continuing, leadership efforts sporadic, and committee obstruction very difficult to overcome. Ironically enough, then, the ultimate result of the revolt of 1910 was to redefine the problem of majority rule in the House, not to solve it. A new and equally serious difficulty, i.e., minority obstruction, simply replaced the difficulty that had aroused passions in the preceding era, i.e., autocratic leadership power.

Leadership Style in the Rayburn House. In sum, by 1940 the role and power of the party leadership in the House had been substantially altered. Though the leadership retained responsibility for and continued to provide overall guidance and direction in the conduct of the House's business, it now had to operate within a far harsher set of constraints than in 1910. At the floor stage, the leadership usually had no choice but to engage in the painful process of assembling shifting majorities behind particular bills through bargaining and maneuver. At the committee stage, the leadership was often forced to engage in intricate and prolonged negotiation with committees and committee chairmen. Indeed, the leadership was now placed in a position where inability to accommodate an organizational unit would mean failure to pass party legislation, unless it was able to organize a majority of such strength and intensity that it could force a vote on the floor through the pressure of opinion in the House or the use of a mechanism such as discharge. The result was that by

1940 the personal, political skills of the leadership, rather than its sources of institutional power, had become the critical determinant of the fate of party programs.

All this, in turn, led to the emergence of a leadership style that contrasted markedly with that of Cannon and Reed. The components of this new style emerged gradually in the 1920s and 1930s as power in the House decentralized. It crystallized under Rayburn and was fully applied by him. It represented his experienced and finely tuned sense of what made for effective leadership in a House in which the Speaker lacked the formal powers of a Czar, had to mobilize a majority party fairly evenly balanced between discordant northern and southern elements, confronted a set of committees and committee chairmen with great power and autonomy, and had to deal with individual members who rejected party discipline and prized their independence.

The main facets of the Rayburn style can be analyzed in terms of the following categories: personal friendship and loyalty, permissiveness, restrained partisanship and conflict reduction, informality, and risk avoidance.

Whereas Cannon and Reed relied on their authority and power as Speakers and party chiefs, Rayburn relied on personal friendship and loyalty. If the Speaker could no longer command the House, his vantage points in the formal and party systems as well as his personal prestige provided a variety of opportunities to do favors for members. Rayburn exploited these opportunities in a skillful and imaginative manner. He sought continually to bind members to him as a person on the basis of favors rendered to them as persons, favors which eased their lives in Washington, enhanced their sense of personal worth, and/or advanced their political careers. In contrast to Cannon and Reed, who emphasized policy goals over personal relationships, Rayburn sought to attain policy goals through personal relationships, through nurturing friendships and creating obligations (Bolling, 1965, pp. 65-68; Daniels, 1946, pp. 56-58).

Whereas Cannon and Reed were quite intolerant of party defection and quite amenable to employing punishments as well as rewards as means of inducement, Rayburn was very permissive. He explicitly legitimized party irregularity on the basis of policy disagreement or constituency pressure and was reluctant ever to punish or coerce a member. To be sure, he did withhold rewards or favors from those he felt failed to cooperate with him for light or insubstantial reasons. Nonetheless, his prevailing inclination was not to alienate members whose vote or help he might need on future occasions (Steinberg, p. 178).

Whereas Cannon and Reed were highly partisan and accepted both intraparty and interparty conflict as necessary aspects of majority party leadership, Rayburn sought to temper partisanship in personal relationships and to restrain conflict generally. He saw party mechanisms, such as the caucus and steering committee, as mechanisms for exacerbating party divisions and studiously ignored them. He established friendly relations with minority party leaders receptive to his overtures and extended advice and favors to rank-and-file minority members. He emphasized reciprocity and compromise as the prime behavioral rules for all members. Thus the guiding motif of his regime was not "serve party policy goals," but rather "to get along, go along," i.e., trade favors (MacNeil, 1963, pp. 84-85).

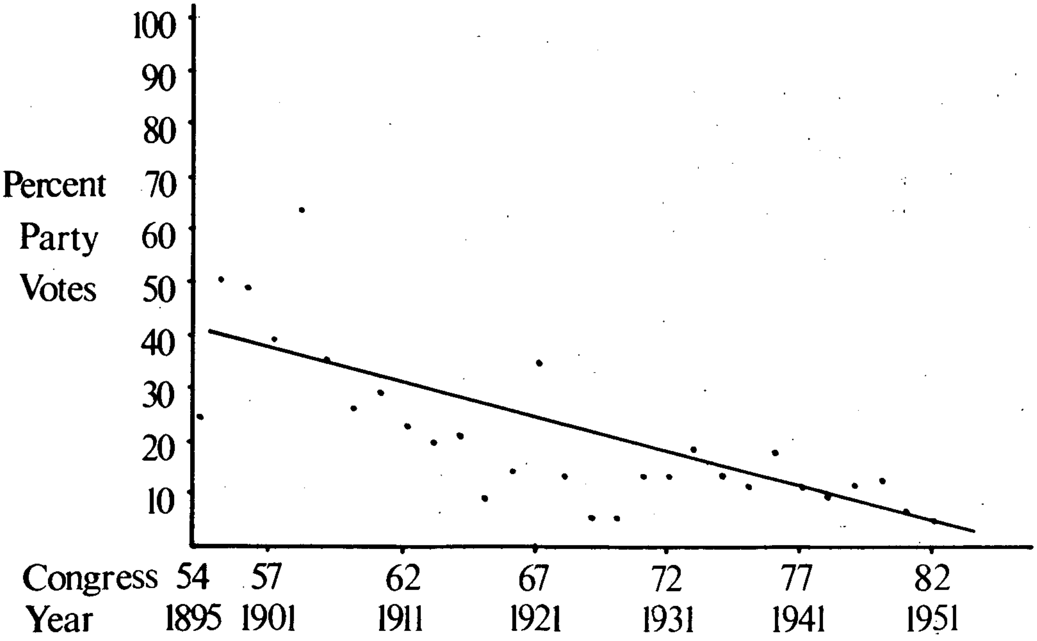
Whereas Cannon and Reed sought to achieve party programs by mobilizing partisan majorities and working through a stable set of partisan lieutenants, Rayburn's approach was more informal and ad hoc. Bargaining needs and opportunities determined his legislative strategies and personal contact served as his main means of implementing these strategies. Thus, on the whole, he worked through varying sets of trusted friends who were loyal Democrats and whom he had placed in key positions in the committee system. However, he was not averse when pressed at the committee stage to appealing to powerful opponents, who were nonetheless close friends, for help, men such as the southern Democratic stalwart, Gene Cox, or the Republican leader, Joe Martin. Similarly, at the floor stage he customarily asked varying sets of members, who were close friends and/or owed him favors, to insure his majority by standing ready to vote for him if needed, even against their policy preferences and/or constituency interests (Clapp, 1963, pp. 286-87).

Finally, whereas Cannon and Reed were aggressive in the pursuit of party policy goals, Rayburn was cautious. His inclination was to avoid battles when the outcome was uncertain. To be sure, in instances when a Democratic president and/or large number of his fellow partisans pressed him, he would usually wage some sort of fight. But both because he felt that defeat undermined his influence and because he did not like to expend his credits in losing causes, his clear and decided preference was to refuse battle, to wait until prospects for victory were favorable. Similarly, he shied away from challenging any of the key facets of decentralized power in the House, despite their restrictive impact on his ability to lead. His inclination was to work with what existed and endure, rather than to seek basic change. Only when extremely provoked did he contest the power of senior chairmen or the prerogatives of the Rules Committee and even then only indirectly. Thus he did not discipline Graham Barden but rather took over the Education and Labor Committee by filling vacancies with liberal Democrats. Thus he did not discipline Howard Smith or Bill Colmer or limit the power of the Rules Committee. He rather chose to expand its membership. In short, then, Rayburn was far more inclined to accept the defeat of party programs than to risk his influence and prestige in battles to attain them (Clapp, 1963, pp. 66-69; Wicker, 1968, pp. 43-54).

The Bases of Personalized Leadership. Earlier we argued that Czar rule derived from the interaction of the Speaker's formal powers and his leverage as party chief. We further argued that party strength was the determining factor in this interaction and that it was rooted primarily in the polarized constituency bases of the two parties. The emergence by 1940 of a new type of House and a new leadership style, both of which we may identify with Sam Rayburn, can be explained in terms of the altered character and impact of these same variables.

Confining ourselves simply to events in the House, the interaction between formal power and party strength again played a critical, though quite different, role. As we have already suggested, the interaction of these variables now worked to reduce leadership power. Party strength could no longer support or justify high concentrations of formal power in the leadership. Limited formal power, however, allowed party divisions fuller expression and increases in these divisions undermined party control mechanisms. The atrophy of these mechanisms, in turn, augmented the power and independence of party factions and transformed the leadership into bargainers and brokers, into middlemen rather than commanders (Truman, 1959, pp. 202-27).

Evidence of the continuing decline in party strength, which we interpret as both cause and effect of the decentralization of power in the House, has already been presented in Table 3. To reinforce our tabular evidence we regressed party voting against time for the whole period from 1894-1952. The results are presented in Figure 1. The slope of the line is negative (B = -1.3) and the correlation between time and party voting -.74. Clearly, changes in party strength and changes in



Intercept = 40.8
B = −1.3
r = − .74

Source: David Brady, Joseph Cooper, and Patricia Hurley, "The Decline of Party in the U.S. House of Representatives, 1887-1968," *Legislative Studies Quarterly* 4 (1979): 384-85.

Figure 1. Party Voting in the House of Representatives (1895-1953)

insitutional structure covary in a manner that is consistent with our argument.

Nonetheless, if we again would acknowledge the impact of the internal, interactive effect between formal power and party strength and accord party strength the determining role in this interaction, we again would also argue that levels of party strength are subject primarily to external determination. In short, though restricted formal power provided a context in which party divisions could be expressed and extended, the primary engine of increased divisiveness was increased disharmony in the constituency bases underlying the majority party coalition. Thus, as in the case of leadership power and style during the period of Czar rule, the key to the Rayburn House and the Rayburn style lies in electoral alignment patterns.

Table 2 shows that from 1881 to 1921 party strength was high when the constituency bases of the parties were highly polarized and that it declined when these bases became less polarized. We have argued that increased factionalization in the party system was the primary source of the increased divisiveness that undermined the use of party control mechanisms in the 1920s. In order to show how constituency alignments are related to the further decline of party and the emergence of the Rayburn House, it is necessary to analyze the New Deal realignment and its aftermath.

The political revolution known as the New Deal was the product of the Great Depression. The voters providing the Democrats their majority came primarily from those groups most affected by the depression, farmers and low-income city dwellers, including blue-collar workers, ethnic groups, and blacks. Thus, the New Deal resulted in an increase in Democratic party allegiance across all constituent characteristics. Rather than recreating polarized congressional parties as in the period of Czar rule, the New Deal created a monolithic majority party which encompassed all types of constituencies. To the Democratic party's traditional base of support, the rural South, it added the urban Northeast and the urban and rural Midwest (Sundquist, 1973, pp. 183-218; Ladd and Hadley, 1975, pp. 31-88).

Table 4 illustrates and supports this point. It includes the following data, collected from the 1930 census and mapped onto congressional districts: the number of blue-collar workers, value added by manufactures, and population density. Constituencies are ranked as high or low in relation to these characteristics in terms of the national mean and the percentage increase as well as the absolute ratio increase calculated for Democratic congressmen.

The monolithic majority party coalition created by the New Deal was formed around the basic issue of government aid to combat the effects of the Depression. Hoover and the Republican party favored voluntarism and nonintervention, whereas the Democrats favored active government involvement. As long as the issue of the role of government in combating the Depression remained the central and defining one, congresional Democrats had a broad basis for unity, despite their increased disparateness. And, indeed, in the 1930s there was a break in the long-term trend toward declining party voting (see Figure 1). However, as is now evident, the Roosevelt coalition could not maintain its cohesion across changing issue dimensions (Sinclair, 1978). As a monolithic rather

Table 4. Increases in Congressional Majority Party Composition during 1932 Realignment

District Characteristic	Percent Democratic Congressmen 70th House (1927-29)	73rd House (1933-35)	Percentage Increase	Absolute Ratio Increase
Blue-Collar				
Low (Farm)	57	82	25	1.37
High (Labor)	32	64	32	1.97
Value Added				
Low (Non-Industrial)	54	81	27	1.40
High (Industrial)	34	67	33	1.89
Density				
Low (Rural)	53	77	24	1.38
High (Urban)	30	67	37	1.96

Source: Compiled from U.S. Bureau of Census, *Fifteenth Census of the United States* (1930): *Agriculture,* Vol. 2, parts 1, 2 and 3; *Manufactures,* Vol. 3; *Population,* Vol. 3, parts 1 and 2 (Washington, D.C.: U.S. Government Printing Office).

than polarized coalition, it was particularly vulnerable to the emergence of issues that would divide its various components rather than unite them as the Depression had done.

In the late 1930s and early 1940s two factors combined to redefine the political climate and render it far less hospitable to majority party unity. The first was the alteration in the character and thrust of the New Deal, which focused attention and controversy on the federal government's general role as an agent of social welfare rather than its narrower role as an agent of economic recovery. The divisive potential of this development was signaled by the battle over New Deal legislation in the 75th Congress (1937-1939), a battle that in the eyes of many analysts marks the true emergence of the Conservative Coalition (Brady and Bullock, 1980). The second was the worsening international situation, which finally led to the Second World War. This development focused attention and controversy not only on defense and foreign policy, but on the management of a war economy as well. In so doing, it also by-passed or submerged old bases of Democratic unity and reinforced divisions along liberal-conservative lines (Young, 1956).

The emergence and growth of the Conservative Coalition in the late 1930s and early 1940s, documented in Table 3, testifies to the impact of these factors in producing a new and enduring split in the congressional Democratic party that was rooted in differences between rural conservative southern constituencies and urban liberal northern ones. Nor is it surprising that as the dimensions of the split increased and finally stabilized, party voting declined. A comparison of the data in Table 3 broadly demonstrates the point; but to pin it down we calculated the correlation (Pearson's r) between Conservative Coalition activity and party voting for the period 1931-1953. The result is an impressive -.67.

In short, then, external factors are of primary importance in accounting for personalized rule as well as Czar rule. In a context in which the interaction of restricted formal power and declining party strength had already combined to disperse power, the emergence of a basic split in the majority party, rooted in constituency differences, further substantially undermined leadership power. Rayburn's highly personalized style was thus a reaction to his party situation, to the corps of independent and divided partisans he had to work with and lead. Indeed, his style is not only distinguishable in kind from that of Cannon and Reed, but even in degree from that of preceding Democratic Speakers, such as Garner and Rainey, who did not have to worry continually about southern support on key committees and the floor (MacNeil, 1963, p. 34).

Conclusion

Our historical analysis of the transition from Cannon to Rayburn suggests several broad propositions or hypotheses that explain House leadership roles and behavior and have general import or significance for the analysis of legislative leadership. They are as follows.

First, institutional context rather than personal skill is the primary determinant of leadership power in the House. To be sure, leadership power, like other forms of power, is a combination of the fund of inducements available and the skill with which they are used. Nonetheless, skill cannot fully compensate for deficiencies in the quality or quantity of inducements. Indeed, the very skills required of leaders themselves vary in terms of the parameters and needs imposed by the character of the House as a political institution at particular points in its history. Thus, Rayburn was not and could not be as powerful a Speaker as Cannon or Reed. His sources of leverage in the formal and party systems were simply not comparable. Nor did Reed or Cannon require the same level of skill in building credits or bargaining as Rayburn to maximize their power. Similarly, it is doubtful that O'Neill can be as strong a Speaker as Rayburn whatever the level of skill he possesses, given the increased fractionalization in both the formal and party systems that has occurred in the past two decades.

Second, institutional context rather than personal traits primarily determines leadership style in the House. To be sure, style is affected by personal traits. Nonetheless, style is and must be responsive to and congruent with both the inducements available to leaders and member expectations regarding proper behavior. Indeed, the personal traits of leaders are themselves shaped by the character of the House as a political institution at particular points in time through the impact of socialization and selection processes that enforce prevailing norms. Thus, if Rayburn was a more permissive and consensual leader than Cannon or Reed, this is not because he was inherently a less tough or more affective person, but rather because of his weaker sources of leverage and the heightened individualism of members. If O'Neill's leadership style is far closer to that of Rayburn, McCormack, and Albert than to Cannon or Reed, this is not attributable to basic personality similarities and differences.[4] It is rather attributable to the fact that the House he leads is far more like theirs than the House during the days of Czar rule. Similarly, though leaders remain distinct personalities (note, for example, O'Neill and Albert), range of tolerance for personal traits or predispositions that conflict with prevailing norms is restricted. O'Neill therefore must and does curb the exercise of new sources of leverage gained since the revival of the majority caucus in 1969 to avoid even appearing like a Czar.[5] In contrast, members who cannot eliminate or temper traits that run counter to prevailing norms are disadvantaged in the pursuit of leadership office. Witness Dick Bolling who would have been far more at home in the House of the 1890s.[6] In a basic sense, then, the impact of context on leadership style has something of the character of a self-fulfilling hypothesis.

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[4] O'Neill's style is more partisan in personal relationships and more consultative or open than Rayburn's. But it remains highly personal, ad hoc, informal, and permissive. See *National Journal* (1977), pp. 940-46; (1978), pp. 4-9, 1384-88; (1979), pp. 1326-31.

Third, there is no direct relationship between leadership style and effectiveness in the House. This is true whether effectiveness is interpreted relatively or absolutely. Interpreted relatively, effectiveness is a matter of the skill with which resources are used, not actual results. However, whereas style is primarily determined by the parameters and needs imposed by the political character of the House as an institution at certain points in his history, particular styles can be applied with varying degrees of skill. Note, for example, the differences between Rayburn, McCormack, and Albert. Similarly, if effectiveness is interpreted absolutely, i.e., in terms of actual results of achievements, there is still no direct or simple relationship between style and effectiveness. On the one hand, there is no one best "style"; the relationship between style and effectiveness is rather a highly contingent or situational variable. On the other hand, even when contexts dictate roughly similar styles, they do not necessarily accord them roughly equal chances of success. Thus, given his sources of leverage in the formal and party systems, O'Neill has little choice but to adopt a leadership style that in many key respects is similar to Rayburn's. Yet, the greater degree of fractionalization in both systems in the 1970s, as opposed to the 1940s, reduces his overall prospects for success in passing party programs. O'Neill therefore is likely to have far less success overall than Rayburn, even though he too leads in a highly personal, informal, permissive, and ad hoc manner.

Fourth, and last, the impact of institutional context on leadership power and style is determined primarily by party strength. To be sure, the degree of organizational elaboration substantially affects the intensity of the demands imposed on integrative capacity and is largely a product of factors other than party strength, e.g., size and workload. Nonetheless, integrative capacity derives or flows primarily from party strength (Cooper, 1975). The higher the degree of party unity or cohesion the more power in both the formal and party systems can be concentrated in the hands of party leaders and the more leadership style will be oriented to command and task or goal attainment. The lower the degree of party unity or cohesion the more power in both the formal and party system will be dispersed and the more leadership style will be oriented to bargaining and the maintenance of good relations. The infrequency of eras of centralized power in the House is thus explicable in terms of the very high levels of party strength required to support it, requirements which have increased as the organization itself has become more elaborate. Similarly, the degree of power dispersion now present is explicable in terms of present weaknesses in party unity or coherence both in absolute terms and relative to an organizational structure that has grown far more complex in the past two decades. Given the dependence of internal party strength on appropriate constituency alignments, all this, in turn, means that leadership power and style are ultimately tied to the state of the party system in the electorate, that external or environmental factors have a decisive bearing on the parameters and needs that institutional context imposes on leadership power and style. In short, then, to be understood Cannon and Reed must be seen in the context of an entire party system, and the same applies to Rayburn in the 1940s and 1950s and to O'Neill today.

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[5] Since the mid-1970s the Speaker has had the power to nominate Democratic members of the Rules Committee to the Democratic Caucus and the power to multiply or sequentially refer legislation to committees, to establish time limits for their reports (in effect, Speaker discharge), and to establish special ad hoc committees to receive such reports and report to the House. However, the Speaker has used ad hoc committees and referral with a time limit cautiously. In addition, he has not as yet refused to recommend the reappointment of any sitting Rules Committee member and threatened to do so only rarely and even then only by implication. See *National Journal* (1977), pp. 940-46.

[6] Bolling is perhaps more reminiscent of Reed than any recent House leader. Compare Polsby (1969, pp. 343-45), Bolling (1975, pp. 236-44), and *National Journal* (1976), pp. 170-71, with Brown (1922, pp. 84-97) and Robinson (1930, pp. 377-99).

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The End Game in Presidential Nominations

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This study shows that a simple, empirically derived rule accounts well for the outcome of multi-ballot, majority-rule conventions in the period 1848-1948 and that the same rule would have enabled one to predict correctly the outcome of all contested presidential nominations since 1952 before any candidate had achieved majority support in the Associated Press' polls of delegates. The rule's success suggests two conclusions about the behavior of delegates: (1) that their guesses about outcomes are based on objective cues, similarly interpreted, and (2) that the trend in voting for candidates, in addition to the level of support they attract, is an important element of such cues. The study's findings also suggest that delegates in recent years do not *contrast sharply with their predecessors in their predilection for bandwagon voting.*

By a usage long established in both American and British politics, the calculated shifting of one's support from a loser to a winner is known as "jumping on the bandwagon."[1] Delegates to U.S. national party conventions often try their skill at this athletic feat, most observers agree, and most also agree on the reason: many delegates seek spoils. Successful presidential candidates, who control a vast treasury of spoils, reward friends and punish enemies. After the fashion of the leprechaun in "Finian's Rainbow," delegates who cannot win with the candidate they love are prepared to love the candidate who wins, and their voting traces patterns of support like those in Figure 1. That this view of bandwagon voting in conventions should be widely accepted is not surprising. The great shifts in support often observed in conventions argue strongly that something other than assessments of candidates' merits guides the voting of many delegates. Conceivably, sudden fits of enthusiasm might set off the stampedes toward the winner that characteristically end conventions, but such an explanation does not square well with what is known about delegates and their leaders.

Note, however, that the common view of convention bandwagons does not fully explain them. It gives the motive for bandwagon voting but not

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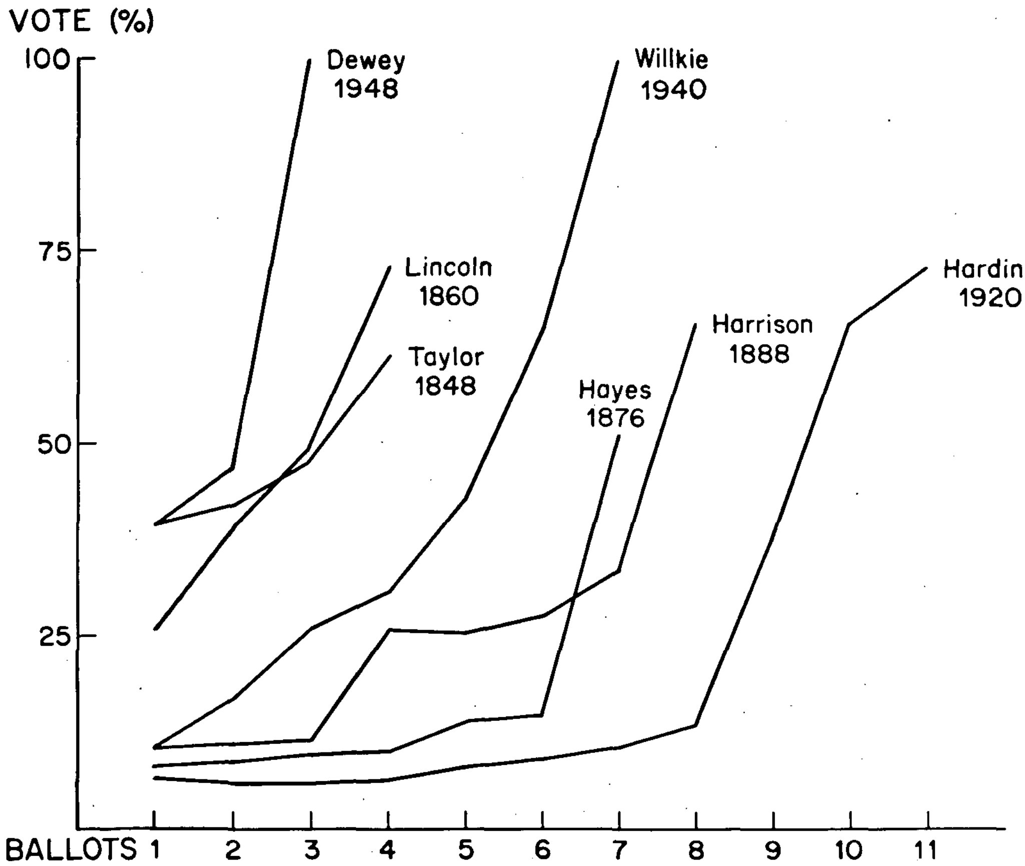
[1] The original meaning of "bandwagon" was literal: "a large wagon, capable of carrying the band in (and typically at the head of) a procession." By the early 1900s in the United States, "keeping abreast of the bandwagon" meant, roughly, being in the van, e.g., of fashion or of opinion, and "to climb, hop, jump, etc., on the bandwagon" came to mean to put oneself at the head of a crowd or, more broadly, "to join in what seems likely to be a successful enterprise, to strive to join the winning side." The first recorded use of the term in its political sense is attributed to Theodore Roosevelt, who, as governor of New York, nominated to the State Lunacy Commission in 1899 one William Church Osborn—"a Princeton man, well up in charitable work," as Roosevelt described him. A nominal Democrat, Osborn was strongly opposed by organization Republicans in the state senate; but, on April 28, Roosevelt was able to write him: "Well, old man, we got you through! At first it looked very doubtful, but when I once became sure of one majority [sic] they tumbled over each other to get aboard the band wagon." See Burchfield (1972, vol. 1, p. 197) and Morison (1951-54, Vol. 2, pp. 985, 999).

the means—an important omission, because profitable bandwagon-jumping requires skill as well as motivation. Candidates are most grateful for support that helps them win; to jump when a candidate has already been nominated is to jump too late for anything but cursory consideration. A delegate can also jump too soon, off a bandwagon only temporarily stalled, or onto one that later stops dead in its tracks, for an overall effect that is graceless. Although timing is everything, when to jump is not obvious, especially if there are many would-be bandwagon riders. Predicting the outcome of a race in which loyalties or ideology tightly constrain choices can be relatively easy, once those constraints are known. Predicting the votes of bandwagon chasers, however, requires a guess about others' guesses, which, as many followers of the stock market can sadly testify, is a perilous undertaking.

Yet the data suggest that delegates' predictions are good ones and that they very largely coincide. Why? How does one guess well in such a case? Without an answer to these questions, the coincidence of judgments in bandwagon voting remains unexplained. We shall suggest a possible answer by showing that anyone could have predicted accurately the outcome of all contested nominations from 1952 onward, using information easily available to any delegate, and could in every case have done so *before* the final rush of support that decided the nomination.

A Predictive Rule

Imagine a delegate, typical of some sizeable group of delegates, who is wholly indifferent to the merits of the candidates and wants only to support the winner before the decisive ballot.



Source: Constructed from data compiled in Richard C. Bain and Judith H. Parris, *Convention Decisions and Voting Records,* 2nd ed. (Washington, D.C.: Brookings, 1973).

Figure 1. Patterns of Support for Winning Candidates in Multi-Ballot, Majority Rule Conventions, 1848-1948

What information might he or she use to guess the eventual winner? In the rumor-ridden environment of a national convention, surely the most reliable indicators of candidates' support are early ballots (McGregor, 1974, p. 470). Our delegate should examine this record closely for some show of strength impressive enough to persuade delegates that a candidate's victory is highly likely—so likely, in fact, that they will jump on that candidate's bandwagon, bringing victory with them.

Three factors might affect delegates' estimates of a candidate's chances:

1. S_{t-1}, the candidate's support in the most recent test of it (on a convention ballot or in a survey of delegates' commitments);
2. $(S_{t-1} - S_{t-2})$, the change in the candidate's support from the next-to-last test to the last one; and
3. Q, the minimum proportion of the vote sufficient to nominate.

The ratio,

$$\frac{S_{t-1} - S_{t-2}}{Q - S_{t-1}},$$

which combines these terms, seems a reasonable measure of how impressively a candidate has performed on any given test of support (Collat, Kelley, and Rogowski, 1976). The ratio takes account of where the candidate is, how close victory is, and how fast the candidate is progressing towards it. A negative value for $(S_{t-1} - S_{t-2})$ means that a candidate has lost support, always a bad omen.[2] If a candidate makes a small gain when close to victory, or a large gain when not,

$$\frac{S_{t-1} - S_{t-2}}{Q - S_{t-1}}$$

(which we shall henceforth call the gain-deficit ratio), will be large and positive. Let us say, then, that a candidate's showing is the more impressive the larger the gain-deficit ratio, and impressive enough to predict victory when that ratio reaches some critical value.

A procedure for our hypothetical delegate to use in forecasting the outcome of a contested convention now suggests itself: compare the amount of support that each candidate still needs to win after the second test of support (the candidate's "deficit") with the change in that candidate's support between the first and the second test. Predict to win any candidate whose gain-deficit ratio attains the critical value. If there is none such, calculate the gains and deficits of the candidates anew after the third test of support. Again, predict to win any candidate whose gain-deficit ratio attains the critical value, and, if there is none, recalculate after the next test. And so on.

There is one ambiguity in this procedure: the gain-deficit ratios of more than one candidate could reach the critical value simultaneously. The natural solution is to predict victory for that candidate with the highest gain-deficit ratio.

Now, our delegate needs one more piece of information to apply the procedure just described: the critical value of the gain-deficit ratio.

We looked for a critical value of the ratio in the voting of one set of conventions—the eleven majority-rule conventions of three or more ballots held before 1952—and then used that value to predict the outcomes of another set—contested conventions from 1952 onward.[3] The critical value that maximized the accuracy of predictions for the conventions before 1952 turned out to be 0.360.[4]

Table 1 reports the results of our procedure when applied to contests in majority-rule conventions before 1952, using 0.360 as the critical value of the gain-deficit ratio. In two of the eleven conventions we were unable to make a prediction; in those no candidate's gain-deficit ratio reached 0.360. In the remaining nine conventions, we predicted all winners correctly. This result was unexpected; before embarking on our search for a critical value, we expected to make at least some incorrect predictions, whatever specific value we chose.

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[2] In majority-rule conventions no winning candidate has sustained successive losses of as much as 1.5 percent of the convention's vote. The highest successive losses for winning candidates who suffered any losses at all were those of Winfield Scott in 1852, 1.4 percent; James A. Garfield in 1880, .3 percent; Benjamin Harrison in 1888, .5 percent, and Warren G. Harding in 1920, .8 percent. Further, no candidate who led on the first ballot of a majority-rule convention ever lost votes thereafter—*any* number of votes on *any* ballot—and then won nomination.

[3] In the period 1848-1948 there were 19 majority-rule conventions of one ballot, none of two ballots, and (as noted) eleven of three or more ballots. In that period Whigs and Republicans required a majority to nominate; the Democrats required a two-thirds majority until after 1936.

[4] There was actually a range of critical values that maximized the number of correct predictions for the eleven conventions before 1952. In choosing a predictive rule to be applied to conventions since then, our main aim was to minimize the potential for error. *Ceteris paribus,* the higher the critical value specified in the rule, the greater the chance that a candidate attaining it would turn out to be the winner. Thus, we chose the highest value of the range.

Table 1. Predicting the Outcome of Majority-Rule Conventions of Three or More Ballots, 1848-1948 (Critical value of gain-deficit ratio = 0.360)

Convention[a]	Prediction Possible?	Predicted Winner	Actual Winner	Actual Value of Gain-Deficit Ratio	Ballot of Prediction	Total No. of Ballots
1848 W	Yes	Taylor	Taylor	2.144	3	4
1852 W	Yes	Scott	Scott	0.429	50	53
1860 R	Yes	Lincoln	Lincoln	1.491	2	4[b]
1876 R	No	–	Hayes	–	–	7
1880 R	No	–	Garfield	–	–	36
1884 R	Yes	Blaine	Blaine	0.722	3	4
1888 R	Yes	Harrison	Harrison	0.607	4	8
1916 R	Yes	Hughes	Hughes	0.453	2	3
1920 R	Yes	Harding	Harding	2.038	9	10
1940 R	Yes	Willkie	Willkie	0.363	3	6
1948 R	Yes	Dewey	Dewey	2.455	2	3

Sources: Richard C. Bain and Judith H. Parris, *Convention Decisions and Voting Records,* 2nd ed. (Washington, D.C.: Brookings, 1973); *New York Times,* 22 June 1852; and *Official Proceedings of the Republican Convention of 1880.*

[a] W = Whig, R = Republican.

[b] The third ballot, in which there was a shift in votes after the roll call, has been counted as two ballots, since no candidate had a majority after the initial roll call.

Table 2 shows the application of our procedure in two cases. On the second ballot of the Republican convention of 1860, Abraham Lincoln's gain was 1.491 times his deficit, and the gain-deficit ratio of every other candidate remained below 0.360. Lincoln should be predicted the winner. On the second ballot of the Republican convention of 1940 no prediction could be made, but on the third ballot Wendell Willkie's (and only Willkie's) gain was more than 0.360 times his deficit. A prediction is again in order. Notice that in both these cases the predictions are made when the outcome is still far from obvious and when a losing candidate still holds the lead.

Predicting the Outcomes of Recent Conventions

In making predictions for recent contests, we treated press polls of delegates' preferences as equivalent to convention ballots. Obviously, the two are not the same thing: the latter decide the nomination; the former do not. Still, our best information about the delegate-strength of candidates before conventions comes from such polls, and there is ample reason to think that delegates take them seriously.

We found seven or more surveys of delegates' preferences for every recent race whose outcome was in doubt after the last primary: the Democratic contests of 1952, 1956, 1960, 1968, and 1972 and the Republican ones of 1952, 1964, 1968, and 1976. All the surveys that underlie Table 3 and Figure 2 were conducted by the Associated Press, except for one conducted in 1956 by the staff of the *New York Times;* and all were taken when delegates representing at least seven-eighths of a convention's votes had been chosen. Our decision to start counting at this precise point was more or less arbitrary. Early in a race for nomination, many increases in candidates' support reflect the decisions of the primary electorates or party agencies that mandate delegates, not those of the delegates themselves. This fact suggested limiting our attention to surveys taken after all delegates had been chosen, but we also wanted to see how early we might be able to predict outcomes. In the end we found that no predictions would change, whether based on surveys taken when as few as 80 percent of the delegates had been chosen or when all had been.

Table 3 reports our results for recent races, using 0.360 as the critical value of the gain-deficit ratio. This time, predictions were possible in every case, and all were correct.[5] Four of the predictions

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[5] We also applied our rule to data from surveys by the *New York Times* in the 1972 Democratic race and by the *New York Times* and the *Washington Post* in the 1976 Republican race. These data also yielded the predictions reported in Table 3, though the gain-deficit ratios and

Table 2. Two Predictions of the Onset of Bandwagon Voting

Republican Convention of 1860 (Needed to Win: 234)

Candidate[a]	Ballot Number and Vote 1	2	3	4
Seward	173.5	184.5 (0.222)[b]	180	121.5
Lincoln	102	181 W[c] (1.491)	231.5	340
Cameron	50.5	2 (−0.209)	0	0
Bates	48	35 (−0.065)	22	0
Chase	49	42.5 (−0.034)	24.5	2

Republican Convention of 1940 (Needed to Win: 501)

Candidate[a]	Ballot Number and Vote 1	2	3	4	5	6
Dewey	360	338 (−0.135)	315 (−0.124)	250	57	11
Taft	189	203 (0.047)	212 (0.031)	254	377	318
Willkie	105	171 (0.200)	259 W[c] (0.364)	306	429	655
Vandenberg	76	73 (−0.007)	72 (−0.002)	61	42	0

Source: Richard C. Bain and Judith H. Parris, *Convention Decisions and Voting Records,* 2nd ed. (Washington, D.C.: Brookings, 1973).

[a]The table omits data on minor candidates.

[b]Gain-deficit ratios appear in parentheses.

[c]W is attached to the tally on which a victory would have been predicted.

—for the Democratic conventions of 1960 and 1972 and the Republican conventions of 1964 and 1976—were based on information available some three to six weeks before these conventions opened.

Figure 2 reports our results in another way. The vertical axis represents votes gained $(S_{t-1} - S_{t-2})$, the horizontal axis the deficit in votes remaining $(Q - S_{t-1})$. For each predicted winner, we have plotted the ordered pair (gain, deficit) associated with that test of support on which he was predicted to win. The other points represent the highest value of the gain-deficit ratio attained by other strong contenders in tests before or on the one on which the winner was predicted, and the diagonal line marks the critical value. The figure makes clear the predictive efficiency of our procedure. Points above the line should be, and are, those of winning candidates; points below the line should be, and are, those of losers. Using a critical value derived from other data and in the face of much happenstance, we were able to make some very close calls.

This last fact, while certainly counting for our procedure, also suggests caution for any inclined to gamble. Credentials contests were crucial to the outcome of the Republican race in 1952 and were arguably so for that of the 1972 Democratic race. Had these contests come out differently, our predictions would probably have been wrong.[6] In a

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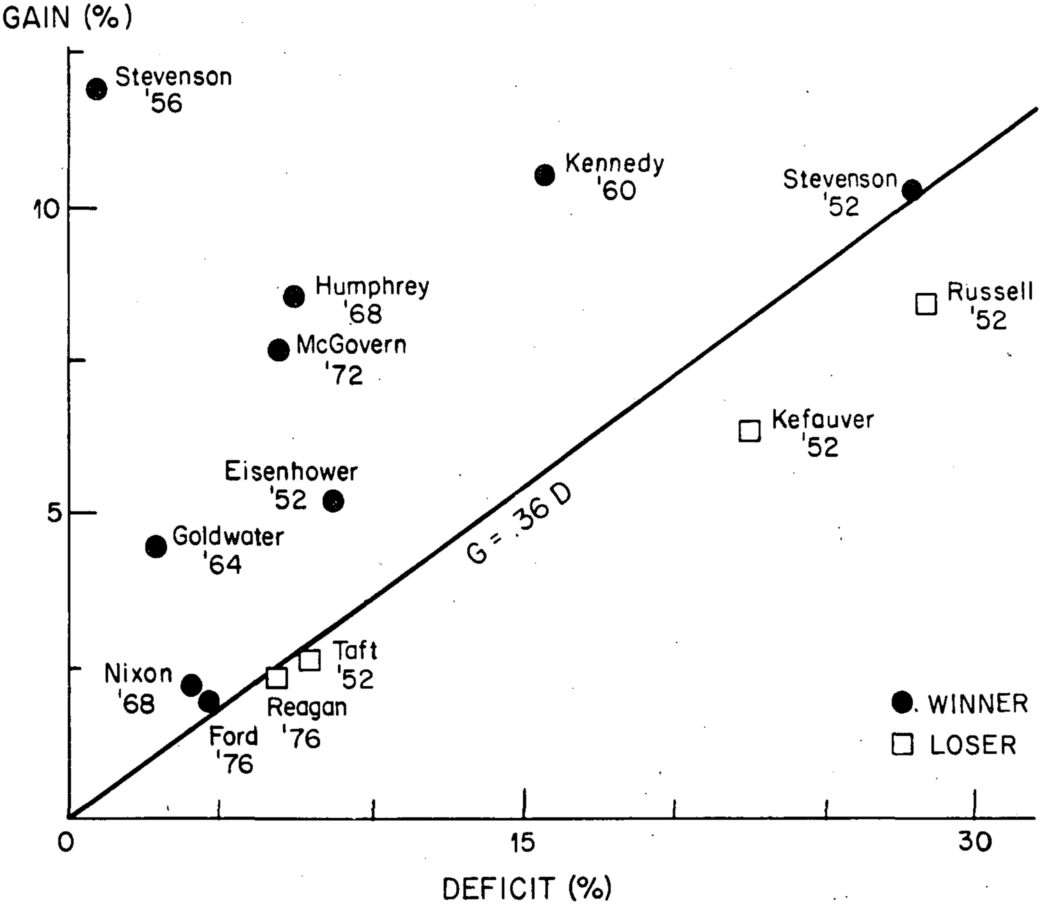
the precise points at which the predictions were made changed somewhat.

[6] Interestingly, in both the *New York Times* and Associated Press surveys McGovern's gain-deficit ratio exceeded 0.360 before the credentials committee (on June 29) had ruled against him on the issue of his entitlement to all the California delegates and again after a court of appeals had ruled in his favor on that issue on July 4.



Sources: Richard C. Bain and Judith H. Parris, *Convention Decisions and Voting Records,* 2nd ed. (Washington, D.C.: Brookings, 1973); Associated Press surveys of delegates reported in the *New York Times* and *Washington Post,* supplemented by Associated Press releases; and a report of a *New York Times* survey, published 29 July 1956.

Figure 2. Winners and Leading Losers, 1952-1976

race as close as the one between Gerald Ford and Ronald Reagan in 1976, small differences in the timing and procedures of surveys could change a prediction without necessarily changing the actual result. In late June of 1976, for instance, the *New York Times* (unlike the *Washington Post* and the Associated Press) began to assign to Reagan the votes of 25 Mississippi delegates. If the *Times* had failed to re-survey between June 27 and June 30, the *Times* data would have forecast a Reagan victory. Further, in early July of 1976 the *Times, Post,* and Associated Press surveys all showed heavy gains by Ford. Aware that time was running out, Reagan announced his selection of a running mate on July 26 and pressed President Ford to do likewise. This maneuver succeeded in unsettling some delegates' preferences, as it was intended to do. While one could argue that the maneuver was doomed to failure because the bandwagon had already begun to roll, slightly different circumstances might have given Reagan the Republican nomination four years before he actually secured it.

As a complication for our procedure, we observed above that the gain-deficit ratios of more than one candidate might reach the critical value simultaneously. In the 20 presidential races now examined, there is no such case. That fact is surely not accidental, since a big gain for one candidate depletes the pool of votes from which big gains for another must come.

In races for the vice-presidential nomination,

Table 3. Predicting the Outcomes of Contested Presidential Nominations, 1952 to 1976 (Critical value of gain-deficit ratio = 0.360)

Convention[a]	Winner Predicted	Gain-Deficit Ratio	No. of Tests of Support	Test No. at Prediction	Prediction Was Possible:
1952 R	Eisenhower	0.602	16[b]	13	One day before balloting
1952 D	Stevenson	0.364	11	9	First ballot (of 3)
1956 D	Stevenson	12.654	8	7	Just before balloting
1960 D	Kennedy	0.672	8	2	2½ weeks before convention
1964 R	Goldwater	1.568	11	2	4½ weeks before convention
1968 R	Nixon	0.519	12[c]	11	Day before balloting
1968 D	Humphrey	1.169	15	13	Day before balloting
1972 D	McGovern	1.113	9	2	3 weeks before convention
1976 R	Ford	0.431	18	4	5¾ weeks before convention

Sources: Richard C. Bain and Judith H. Parris, *Convention Decisions and Voting Records,* 2nd ed. (Washington, D.C.: Brookings, 1973); Associated Press surveys of delegates reported in the *New York Times* and *Washington Post,* supplemented by Associated Press releases; and a report of a *New York Times* survey, published 29 July 1956.

[a] R = Republican, D = Democrat.

[b] The first ballot in which there was shift in votes after the roll call has been counted as two ballots, since Eisenhower fell short of a majority before the shift.

[c] A shift after the roll call has not been counted as a separate ballot, since a majority was attained before the shift.

however, there have been three cases of this sort.[7] In each the second ballot saw two contenders attain gain-deficit ratios above the critical value, as follows:

1924 Republican convention—Lowden, 1.345; Burton, 0.558
1948 Democratic convention—Truman, 1.417; Wallace, 0.375
1956 Democratic convention—Kennedy, 4.688; Kefauver, 0.627.

Our rule for this eventuality—predict to win the candidate with the largest gain-deficit ratio—thus produces two correct predictions and one that is wrong.[8]

Before evaluating our results further, we note a rule of thumb that our hypothetical delegate might use. The number 0.360, which we have used as the critical value of the gain-deficit ratio, is teasingly close to the number one-third. What would happen if one-third were substituted for 0.360?

There would be little change in the accuracy of our procedure. As before, one could predict correctly the winners in all but two of the pre-1952, majority-rule conventions of three or more ballots. One could also make predictions for all the recent conventions that figure in Table 3, and in only one—the Republican convention of 1952—would the prediction be in error.

Some Other Procedures for Prediction

Before adopting our procedure, our hypothetical delegate might compare its results with those of other systems of prediction. Four alternatives are general enough in application to make comparison fruitful.[9] Respectively, they would predict that candidate to win:

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[7] Note that races for second place on a party's ticket are not strictly comparable to those for presidential nomination. A vice-president is not a major dispenser of spoils, and close contests are likely to develop only when the successful presidential candidate, who *is* looked to for spoils, indicates no firm preference for a particular running mate.

[8] The circumstances of the 1956 Democratic race are worth recalling. When the roll call ended on the second ballot, Kefauver's vote stood at 551½ and Kennedy's at 618, with 687 needed to nominate. Senator Albert Gore, Kefauver's junior colleague from Tennessee and the candidate who had run third, then asked his supporters (who included the Tennessee delegation) to support Kefauver, who then prevailed. Analyzing this case, Philip D. Straffin, Jr., shows how a Kefauver victory follows from the assumptions of his model and from the data, once one rules out the possibility of a Gore-Kennedy coalition (Straffin, 1977).

[9] A number of scholars have exploited coalition theory to identify takeoff points for bandwagon voting. See, for instance, Brams and Riker (1972), Brams and

1. who first wins the support of a majority in press polls of delegates;
2. who first receives the support of delegates with 41 percent of a convention's votes;[10]
3. whose gain in votes from the first to the second test of support is greatest;[11] or
4. who has a plurality on the first test of support.

We will call the first of these procedures *the polling-majority rule,* the second *the threshold rule,* the third *the rule of second-ballot gains,* and the last *the front-runner rule.*

Table 4 shows the results of these procedures when applied to contests in which a majority was required to nominate. The first, the polling-majority rule, has an obvious flaw—it yields predictions for only four of the nine recent races. For conventions in the century from 1848 to 1948, the threshold rule has the same shortcoming, though to a lesser extent: it yields seven predictions for eleven cases. One of these, however, is non-unique (Winfield Scott and Millard Fillmore each had more than 41 percent of the vote on the first ballot of the Whig convention of 1852), and another is wrong. The threshold rule holds relatively well for recent conventions: it fails to predict in only one case and yields only one incorrect prediction (for the Taft-Eisenhower race of 1952).[12] Eugene McGregor's "rule of second-ballot gains" permits a prediction in all cases, but five of its eighteen predictions are wrong, and one (that for the Republican convention of 1880) is non-unique. Moreover, in recent races the success of this rule is affected considerably by which surveys one chooses to regard as the first and second "ballots."[13] The front-runner rule is quite successful for recent conventions—predictions in all cases, errors in only two—but very poor for earlier ones—seven errors in eleven cases. Our procedure is less risky than these alternatives.[14]

Having glanced briefly above at the results of contested vice-presidential nominations, we will note here that in all only five such races have gone to more than two ballots in majority-rule conventions: the Whig contest of 1844, the Republican ones of 1868 and 1924, and the Democratic ones of 1944 and 1956. In predicting the outcomes of these, our procedure is correct in four cases, wrong in one. The rule of second-ballot gains does equally well, while the front-runner rule gives three correct and two incorrect predictions, and the threshold rule yields only three predictions altogether, two correct and one incorrect. The polling-majority rule is of course inapplicable to these data.

Concluding Observations

We have shown that an empirically derived rule accounts well for the outcome of multi-ballot,

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Heilman (1974), Brams and Garriga-Pico (1975), Brams (1978), and Straffin (1977). The models developed in these works are of considerable interest, and Straffin's analysis of the 1976 Republican race is particularly suggestive. The assumptions of these models are too restrictive, however, to permit their application to the full set of conventions that we have been analyzing.

[10] This rule adopts and adapts one suggested by Gamson (1962, pp. 65-66), who argued: "What percentage of votes is necessary at a political convention to 'stampede' the delegates for a particular candidate? . . . In majority conventions, the opposition has held firm when a candidate has had as much as 40 percent of the vote but never with more than that in the conventions studied. . . . A candidate with 41 percent or more of the votes when no other candidate has a total close to this generally will have demonstrated to the satisfaction of most of these uncommitted delegates that it is now only a matter of time until he secures the additional votes formally necessary for his nomination." Except for its escape clause concerning other candidates, this procedure amounts in our terms to predicting to win any candidate whose deficit is nine percent or less.

[11] Eugene B. McGregor, Jr. (1974, p. 470) has offered this rule, stated as follows: "The candidate who gains the largest share of the votes from the first to the second ballot will be the eventual winner no matter how many ballots are needed for nomination."

[12] Note that the threshold rule gives an erroneous prediction in the 1952 Republican race and a correct prediction in the 1976 Republican race *as we have stated it.* As Gamson stated the rule (see n. 10), it would yield non-unique predictions for both cases.

[13] We have applied all these alternative rules to exactly the same data to which we applied our own, which means that the first data point was from a survey taken when the roster of delegates was nearly complete, but not entirely so. As noted above, our precise starting point was not important for our results, nor was it in the application of the frontrunner, threshold, and polling-majority rules. Two of the successes and one of the failures of the McGregor rule, however, did depend on the precise starting point that we chose.

[14] One might claim that the success of our procedure in predicting winning candidates is not equivalent to predicting bandwagon voting in those cases in which the initial front-runner did not win. Such a claim would be consistent with a common definition of the bandwagon effect. We do not think this definition a useful one, and it should be distinguished from the one on which we rely: A *bandwagon effect* may be said to exist if some decision maker supports, not the contender he actually prefers, but one who seems more likely to win, and does so *because* that other contender seems more likely to win. This definition is as consistent with the usage described in n. 1 as the more limited sense of the term just noted.

Table 4. Five Procedures for Predicting the Outcomes of Majority-Rule Conventions

C = Correct prediction
E = Incorrect prediction
NP = No prediction possible
N-UP = Non-unique prediction
NA = Data for prediction not available

D = Democratic
R = Republican
W = Whig

Front-Runner Rule		Threshold Rule[a]				Rule of Second-Ballot Gains			Poll Majority			Gain-Deficit Rule[b]	
C	E	C	E	N-UP	NP	C	E	N-UP	C	NP	NA	C	NP
1848W 1884R 1916R 1948R 1956D 1960D 1964R 1968R 1968D 1972D 1976R	1852W 1860R 1876R 1880R 1888R 1920R 1940R 1952R 1952D	1848W 1860R 1884R 1940R 1948R 1956D 1960D 1964R 1968R 1968D 1972R 1976R	1880R 1952R	1852W	1876R 1888R 1916R 1920R 1952D	1848W 1852W 1860R 1884R 1916R 1940R 1948R 1952D 1956D 1960D 1964R 1968R 1968D 1972D	1876R 1888R 1920R 1952R 1976R	1880R	1964R 1968R 1968D 1972D	1952R 1952D 1956D 1960D 1976R	1848W 1852W 1860R 1876R 1880R 1884R 1888R 1916R 1920R 1940R 1948R	1848W 1852W 1860R 1884R 1888R 1916R 1920R 1940R 1948R 1952R 1952D 1956D 1960D 1964R 1968R 1968D 1972D 1976R	1876R 1880R

Sources: Richard C. Bain and Judith H. Parris, *Convention Decisions and Voting Records,* 2nd ed. (Washington, D.C.: Brookings, 1973); *New York Times,* 22 June 1852; *Official Proceedings of the Republican Convention of 1880;* Associated Press surveys of delegates reported in the *New York Times* and *Washington Post,* supplemented by Associated Press releases; and a report of a *New York Times* survey, published 29 July 1956.

[a] Threshold = 41.0%

[b] Critical value = 0.360

majority-rule conventions in the period 1848-1948 and that it also accurately predicts the actions of recent conventions. This rule, simple in form, relies not at all on data regarding the ideological affiliations or factional loyalties of delegates; instead, its key element is an event best interpreted as a signal that a particular candidate is likely to win.

Of what relevance is this finding to what real delegates do in real national conventions? Since all delegates do not jump on the bandwagon simultaneously, it is evident that some either do not follow precisely our procedure for predicting the final rush of bandwagon voting or do not vote in accordance with their predictions. It is reasonable to suppose, however, that some delegates intuitively follow a procedure similar to ours in assessing the odds. If bandwagon-jumpers guessed the outcome of contests idiosyncratically, it is doubtful that any procedure could predict their behavior. Our success in predicting actual results, therefore, suggests two conclusions about competition in the convention setting: (1) that delegates' guesses are often based on objective cues, similarly interpreted, and (2) that the *trend* in voting for candidates, in addition to the level of support they attract, is an important element of such cues.

Interpreted with common sense, our findings are consistent with the general conception of delegates' behavior advanced by James P. Zais and John H. Kessel (1973, p. 124). As they see it, a successful candidacy first attracts its ideologically "natural friends," then, as the coalition supporting it grows, depends more and more on size to attract adherents. In the early phases of a race, changing odds will favor shifts of support within groups of like-minded delegates. At the end, delegates, motivated both by sour grapes and by the sweet smell of success, will move toward the eventual winner, led by those with the weakest preferences, the strongest ambitions, and (perhaps) the keenest desire for party unity. If one is to account for the smoothly increasing support received by almost all nominees, however, delegates must have some shared sense of what impends, some roughly equivalent way of estimating the odds.

Our findings also suggest that the collective behavior of delegates in recent years does not contrast sharply with that of their predecessors in the end game of convention politics. Before and after 1952 the same kind of event forecasts the collapse of opposition to the eventual winner. While the number of "purists" among the delegates may have increased, and while multi-ballot conventions have disappeared since 1952, neither phenomenon is inconsistent with a decisive role for bandwagon riders. Writing in 1960, Paul T. David and his associates observed (1960, pp. 319-20):

> Advance polls of delegates (like those conducted by the Associated Press) . . . may themselves influence the outcome. When they reveal that sentiment is turning strongly toward a single outstanding candidate, they accelerate a development that would otherwise take place more slowly. . . . When one candidate is showing strength in delegate polls, other candidates are under pressure to make a similar showing by urging supporters to become publicly committed. . . . Open contests thus tend to develop more openness of information and the stage is set for bandwagon movements even before the actual voting in the convention.

Our results for recent conventions, reached by treating polls of delegates as the equivalent of early ballots, add considerable force to this observation of 20 years ago. Helped along by the press, as is so much else in contemporary politics, the bandwagon still rolls.

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Economic Expectations and Voting Behavior in United States House and Senate Elections

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Past individual-level studies of economic voting (1) have incorrectly operationalized the model they employ by using past-oriented rather than future-oriented questions and (2) have failed to examine the level of economic voting in United States Senate elections. Using the 1978 National Election Study, we show that economic voting exists in Senate but not House elections, presumably due to the differences in electoral context. Even when economic voting occurs, however, there is no guarantee that the public will influence the direction of macroeconomic policy.

Social scientists have given much attention, particularly during the past five years, to the relationship between American voters' personal economic circumstances and their political expression of them at the ballot box. Early efforts to ascertain the extent of voting based on economic concerns looked at the relationships between aggregate economic indicators and election returns over time. Although far from unanimous, the studies on the whole showed that an economic upturn helped congressional candidates of the incumbent party, while economic decline benefited the opposition (cf. Kramer, 1971; Tufte, 1975).[1] Aggregate electoral outcomes do not uniquely specify or describe the behavior of individual voters (Tufte, 1975, p. 826), however, and so more recent research has turned to the national surveys of the Center for Political Studies (Fiorina, 1978; Kinder and Kiewiet, 1979; Weatherford, 1978). Contrary to the findings of the aggregate studies, investigations based on individual-level data have uncovered only scant evidence that voters' assessments of their personal economic situations influence their choices of congressional candidates. Midterm elections, in particular, appear to fall short of being referenda on the incumbent party's economic performance. Thus Kinder and Kiewiet conclude (1979, p. 523): "Private economic experience is important, but not for politics. Economic discontents and political judgments inhabit separate domains."

The undeniable thrust of the individual-level studies notwithstanding, researchers persist in their efforts to identify economic determinants of the congressional vote. This persistence stems in part from a belief by scholars that whom the electorate puts in office can make a difference. Political choices presumably condition macroeconomic outcomes. Another and probably more telling explanation of researchers' pertinacity is that while individual-level data are better suited than aggregate data to uncovering a relationship between economic conditions and citizens' choices of candidates, the results to date run counter to conventional political wisdom. Members of Congress, especially those of the administration's party, view a deteriorating economy much as a parent might view an impending flu epidemic—with fear and trepidation.

So who is right—social scientists armed with survey data or the politicians who must place themselves directly before the voters? Believing as Kernell did in his earlier work (1977) that conventional wisdom should not be taken lightly, we argue below that conclusions about the absence of a linkage between personal economic circumstances and congressional vote may be premature.

While the study of economic voting is important in its own right, it is only part of a more general concern, namely, whether members of the public exert at least some control over the direction of economic policy. Understandably, efforts

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[1] A complete review of the literature on this topic is beyond our scope here, but is aptly summarized in most work published within the last two years.

to answer this more general question should begin with the study of economic voting; after all, nothing is more fundamental to popular control than the idea that citizens hold officials accountable for their collective actions. But voting on economic grounds is not tantamount to popular control of economic policy. Even when citizens carry out their democratic responsibility, other constraints may limit if not preclude their abilities to affect the macroeconomic performance of government. While it is easy to overlook these constraints when a researcher uses individual-level data, failing at least to take cognizance of them can lead to inappropriate—in particular, overly optimistic—conclusions about how much influence citizens do in fact exert. Accordingly, when we consider the implications of our analysis, we will try to consider some of the constraints on voters' efforts to bring about economic change at the ballot box.

Two Unsettled Matters

Economic Voting as a Prospective Act. The so-called economic voting model is based on the (sometimes implicit) assumption that voters act rationally, which means that they seek to maximize their expected utility (Downs, 1957). Of direct interest here is the term *expected* utility: it emphasizes that as purposive actors, voters are future-oriented in their political decision making, i.e., they base their election decisions on what lies ahead. Specifically regarding economic voting, the congressional vote should be a function of whether voters expect their personal economic situations to improve or deteriorate in the future.

The idea that economic voting as a rational act looks toward the future is clearly evident in the aggregate studies. Arcelus and Meltzer (1975, p. 1233), for example, explicitly use expected utility as an organizing device. Stigler (1973, p. 165), although highly critical of Kramer's pathbreaking study (1971), nonetheless pointedly agrees with him that economic voting is essentially a "forecasting" process. These studies' reliance on indicators of *past* economic conditions—inflation, unemployment, or change in real income, all measured over the past year or two—simply reflects the fact that investigators who use aggregate data are not able to measure voters' expectations directly. They are required to make the assumption (Kramer, 1971, p. 134) that "expectations about year *t* are formed on the basis of experience during the previous year, *t*—1."[2]

It does not necessarily follow, however, that citizens' assessments of past economic circumstances and their expectations about the future will be identical, or even nearly so. To take one real-world example, predictions of a severe recession have increased in number and intensity at the time of this writing. These prognostications undoubtedly have led numerous members of the mass public to conclude that even if conditions have been acceptable up to now, they are about to worsen. In this instance, the researcher's choice of time frame—past or future—could obviously affect conclusions about the impact of economic grievances on congressional voting. More generally, Fair's finding (1978) that the discount rate of voters is high suggests that attempts to incorporate previous periods into the voting model may have limited utility for estimating voter expectations.

The strong possibility of divergence takes on added significance when we turn to the individual-level studies. Although analysts using individual-level data do not lack information on citizen expectations, they have followed in the path of the earlier aggregate studies and also used evaluations of past economic conditions to predict the congressional vote. Typically, they employ this or a similar question: "During the last few years [or in the past year], has your financial situation been getting better, worse, or stayed about the same?"

Because appropriate questions are available, researchers using individual-level data should turn what has been a perhaps unjustified working assumption into an empirical question: *are* voters' evaluations of past economic well-being strongly related to their expectations about future conditions or not? If the two are strongly related, as the aggregate studies *must* assume and the individual-level studies *have* assumed, then the choice of questions should make little difference in terms of the final results. But if judgments of past and future conditions diverge, then researchers should use future-oriented questions that validly operationalize the voting model they employ.[3]

Economic Voting in Electoral Context. A second concern is the failure of researchers to examine the level of economic voting in Senate elections;

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[2] Because of this necessary assumption, researchers using aggregate data have become embroiled in something of a controversy over whether voters look more at the *level* of economic activity or its *change*. This controversy reflects the inevitable frustration involved in attempting to measure expectations indirectly with aggregate data.

[3] Our purpose is not to reject the concept of retrospective voting. The point is that the model of concern is a forecasting model. Fiorina's impressive expansion of his original analysis of economic voting (1980) is an effort to incorporate both types of judgments.

virtually all voting research has focused on the House. So singular has been this focus that the terms "House election" and "congressional election" are now used almost interchangably. Yet there are indications that House and Senate elections differ considerably; these differences support the proposition that economic voting should be greater in one electoral context than the other.

The most widely studied phenomenon in recent years is the so-called incumbency effect (Erikson, 1971). Studies of the electoral advantage of incumbency show a consistently more clear-cut advantage for House than for Senate incumbents. In recent years, the gap has widened (*Congressional Quarterly,* 1979b, p. 1351). This difference in incumbency success raises the very real possibility that House members have more successfully than their Senate counterparts isolated themselves from citizen reaction to in-party economic performance, principally by establishing their roles as ombudsmen and providers of individualized services.

The recent work of Parker and Davidson (1979) captures well the essence of the electoral relationship between House incumbents and their constituents. In an attempt to resolve Fenno's discovery (1975) that voters love their individual members of Congress but hate Congress, they show that the electorate judges Congress in terms of its ability to solve national problems, while it simultaneously evaluates individual members of the House in terms of their reputed ability to provide individualized services. This, they argue, has come about because incumbent House members are able to focus constituents' attention on activities and personal attributes that will enhance their popularity. In other words, they successfully "control" the information—district-oriented and nonpartisan in nature—that voters use to judge them.

If one side of the coin is that the incumbency phenomenon may *discourage* economic voting in House elections, then the other side is that the nature of Senate campaigns may *encourage* it. Because our understanding of Senate campaigns and elections is still in its infancy, the evidence is indirect at best. Nonetheless, Hinckley (1979) has demonstrated that House challengers are considerably less visible to the electorate than Senate challengers, which helps to explain, in turn, why Senate elections are more competitive. Similarly, citizens are more likely to express negative feelings toward Senate than House incumbents, a fact apparently due to the more controversial nature of Senate campaigns (Abramowitz, 1979). Taken in conjunction, these research findings leave the impression that discussions of salient problems and solutions to them are an integral part of most Senate campaigns. When issues, and—in light of our specific concern—economic issues in particular, are forcefully debated, voters should at least become more sensitized to the connection between their own economic situations and the candidate of the in-party.[4]

The observations of practitioners strike the same theme. Democratic pollster Michael Barone has argued, for example, that senators "are just high enough on the political ladder for voters to throw them out of office as an expression of political anger or frustration" (*Congressional Quarterly,* 1980, p. 906). And William Hathaway, former senator from Maine, has noted that "Senators are perceived [by voters] as being more responsible [than House members] for what is wrong" (*Congressional Quarterly,* 1980, p. 906). Again, the implication is that the tie between important issues and in-party candidates is strong in Senate elections, presumably due to the nature of Senate campaigns.

Our intention is neither to elaborate in detail the differences between House and Senate elections nor to explain them. We *are* suggesting that any conclusion about the presence or absence of economic voting in congressional elections must be based on an analysis of both House and Senate voting patterns. For there is ample reason to believe that the nexus between personal economic situation and political expression will be greater in one electoral context than the other.

The 1978 CPS Election Study

To address the concerns posed above, we use data collected in the 1978 National Election Study. Designed explicitly as a study of congressional voting, the 1978 survey serves our purpose in several ways. In light of Fiorina's conclusion (1978) that voters' personal economic situations and their congressional votes are consistently unrelated in off-year elections, the survey's midterm timing provides a stern test of the economic voting thesis. For the first time in some years, moreover, economic issues were the singularly important concern of the mass public. With Watergate and Vietnam behind us and race relations at least temporarily put aside, the state of the economy became the focal point of political attention in 1978. As a statistical matter, the absence of noneconomic factors renders specification error a less worrisome problem.

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[4] It might be argued that this statement runs counter to the notion of economic voting, which, as originally construed (Kramer, 1971), requires little information on the part of the voter. Given the consistent presence of economic voting in presidential elections, it is reasonable to assume that competitive and visible campaigns can at least provoke voters to think in terms of blame.

Another reason to use the 1978 survey is that party control of government was total at the time the data were collected. Most previous investigations—the longitudinal studies in particular—cover periods in which there has been divided control of Congress and the presidency. Researchers have assumed that under such circumstances voters equate the incumbent party with the incumbent president's party, presumably because they see the chief executive as responsible for the economic health of the nation. In light of evidence that voters do not agree on who controls economic policy—the president, Congress, or both—the safest research strategy is to avoid *any* assumption about the most likely target of citizen wrath, which can be done only when the same party controls both institutions.[5]

The data have some limitations, however. One disturbing feature is the rather dramatic discrepancy between the vote reported in the National Election study and the actual vote. Fifty-four percent of the sample indicate they had voted in 1978, while the estimate of the actual vote is 35.1 percent (*Congressional Quarterly,* 1979a, p. 574). This considerable difference between reported and actual turnout, which is characteristic of all SRC-CPS surveys, reflects both an unwillingness by some respondents to admit their absence from the polling booth and a reluctance to participate in an election study among citizens with little interest in politics (Traugott and Katush, 1979). Unfortunately, the discrepancy is a form of systematic error, and there is no foolproof way to estimate its effect on the coefficients we present below.

To complicate matters further, Jacobson (1980) has convincingly argued that the House elections in the sample overestimate the strength of incumbents, while Mann and Wolfinger (1979) have shown that the CPS respondents who voted in Senate elections are concentrated in three states. Because we wish only to examine the level of economic voting within two different electoral contexts, these sampling problems are not prohibitive. From our theoretical perspective, the subsamples of House and Senate voters function essentially as a dichotomous variable, in accord with the chamber differences that we noted in the previous section. On the other hand, the National Election Study probably is not a good source of information from which to generalize about all 1978 House and Senate races. Accordingly, we refrain from making grand statements about the 1978 congressional elections.

Personal Financial Condition and the Midterm Vote

The economic voting model presumes a relationship between expected financial well-being and vote. Because previous research has relied on assessments of respondents' past financial condition, the first question we must consider is whether retrospective and prospective judgments are sufficiently related such that one can justifiably be substituted for the other.

Two CPS items allow us to address this concern; they are as follows:

- We are interested in how people are getting along financially these days. Would you say that you are better off or worse off financially than you were a year ago?
- Now looking ahead—Do you think that a year from now you will be better off financially, or worse off, or just about the same as now?

Both questions obviously deal with personal financial conditions; the first measures perceptions of immediately past trends, the second expectations about the upcoming year.[6] Table 1 reports the relationship between answers to these two questions among individuals reporting to have voted in 1978. Since Senate elections were not held in all House districts for which there are respondents, we have presented the results for each of the two subsamples of voters.

Voters' estimates of past and future financial well-being fall far short of being identical. In terms of those voting in House elections, 51 respondents or 5.2 percent of the total subsample who feel their financial circumstances are better than they were a year ago also predict that they will be worse in the future. At the other extreme, 4 percent of the respondents believe that even though conditions have gotten worse, they are likely to improve in the future. More generally, nearly 50 percent of the respondents do not fall on the diagonal, i.e., their perceptions of past and future financial well-being diverge. Similar patterns hold among the Senate subsample.

Although it does not necessarily follow that the use of retrospective and prospective judgments will lead to different conclusions about the presence or absence of economic voting, the possibility clearly suggests itself. One particularly valuable technique for gauging the impact of each type of judgment on citizens' political decisions is log-linear analysis. Increasingly used by sociologists, log-linear analysis was developed specifically to provide concise measures of the relationships among categorical variables such as those comprising the economic voting model (Goodman, 1972, 1973; see also Knoke and Burke, 1980).

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[5] As part of the 1978 CPS survey, respondents were asked to indicate who, the president or Congress, has more say in handling the two major economic problems facing the country—inflation and unemployment. In both areas, only about 15 percent agree with the traditional assumption that the president has more say; another one-third express just the opposite view; and the large bulk believe that responsibility is equally divided.

[6] To assure ourselves that these two measures do not simply reflect socioeconomic status, we examined the relationships between them and selected characteristics of the respondents. Associations tended to be weak.

Log-linear analysis consists of two logically and practically distinct steps. In the first step, the researcher uses odds and conditional odds ratios to select the most parsimonious and theoretically interesting model from a set of hierarchical models.[7] A well-fitted model is one in which the predicted and actual cell frequencies are similar. The second step involves estimating the strength and direction of the relationships among the variables comprising the chosen model. Either multiplicative or additive parameters can be used to estimate the relationships, although the tendency is toward the latter.[8] When one of the variables is construed as a dependent variable, as is the congressional vote, the log-linear model is conceptually equivalent to a logit model and the appropriate additive parameters are betas (ßs). The ßs are not as readily interpretable as those in regression analysis, but their magnitudes can be compared to ascertain the relative strengths of the relationships.

Table 2 lists, for contested House and Senate elections, a number of log-linear models in which vote is the dependent variable and region and partisan identification, along with past financial condition, are independent variables. Two brief comments about them are in order. First, even though we will focus our attention only on those parameters involving the dependent variable, each of the models includes a term (RPF) that incorporates all possible interactions among the independent variables. This procedure is analogous to regression analysis, which takes the correlations among the independent variables into account even though these relationships are not explicitly included in the equation (Knoke and Burke, 1980, pp. 24-25). Second, out of the complete set of hierarchical models, we have limited analysis to the models that are directly relevant to our theoretical concern of whether personal economic

Table 1. Relationship Between Past and Expected Financial Well-Being Among Voters in House and Senate Elections

Voters' Past Financial Well-Being	Voters' Expected Financial Well-Being					
	House Elections			Senate Elections		
	Better	Same	Worse	Better	Same	Worse
Better	130 (13.3)	20 (2.0)	51 (5.2)	70 (12.8)	85 (15.7)	15 (2.8)
Same	173 (17.7)	199 (20.3)	106 (10.8)	10 (1.8)	134 (24.7)	41 (7.6)
Worse	39 (4.0)	59 (6.1)	202 (20.6)	27 (5.0)	60 (11.1)	100 (18.5)
		Gamma = .56 Tau$_b$ = .39			Gamma = .57 Tau$_b$ = .38	

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

Note: Figures represent absolute frequencies; percentage of total subsample is given in parentheses.

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[7] A hierarchical model obeys the rule that if a parameter involving a set of parameters S is included in the model, then the model must also include all the parameters involving any subset of S. Consider for example, the three-variable model

$$F_{ijk} = \eta \tau_i^{A} \tau_j^{B} \tau_k^{C} \tau_{ij}^{AB} \tau_{ik}^{AC} \tau_{jk}^{BC} \tau_{ijk}^{ABC},$$

where F_{ijk} is the expected cell frequency in cell $_{ijk}$ and the parameters τ_i^A, τ_j^B, τ_k^C represent the main effects or marginal distributions of each variable and τ_{ij}^{AB}, τ_{ik}^{AC}, τ_{jk}^{BC}, τ_{ijk}^{ABC} represent two- and three-way interactions. This model, called the saturated model because it includes all possible interactions and thus must perfectly fit the data, must also include the model

$$F_{ijk} = \eta \tau_i^{A} \tau_j^{B} \tau_k^{C} \tau_{ij}^{AB} \tau_{ik}^{AC} \tau_{jk}^{BC} \tau_{ijk}^{ABC}.$$

[8] The multiplicative model is transformed into an additive one by taking natural logarithms.

Table 2. Log-Linear Models for Vote, Region, Partisan Identification, and Past Financial Well-Being Among Voters in House and Senate Elections

Model	Likelihood-Ratio Chi-Square House Elections	Likelihood-Ratio Chi-Square Senate Elections	P-Value House Elections	P-Value Senate Elections	Degrees of Freedom
(VRPF)	0.00	0.00	–	–	0
(RPF) (VRP) (VPF)	2.96	2.83	>.5	>.5	6
(RPF) (VRP) (VF)	9.56	8.05	.480	>.5	10
(RPF) (VPF) (VR)	4.47	9.70	>.5	.286	8
(RPF) (VPF)	20.39	10.04	.016	.347	9
(RPF) (VR) (VP) (VF)	11.21	14.36	>.5	.278	12
(RPF) (VR) (VP)	18.63	17.82	.180	.215	14
(RPF) (VP) (VF)	26.52	14.64	.014	.330	13
(RPF) (VP)	34.15	18.03	.003	.261	15
(RPF) (V)	264.30	156.09	.000	.000	17

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

Key: V = Vote
R = Region
P = Partisan Identification
F = Past Financial Well-Being

conditions independently affect House and Senate votes. In essence, we have let theory dictate the models to test rather than use every model and simply let the data choose the best-fitting one.

The simplest model is (V). Fitting only the marginals of the dependent variable, it hypothesizes no significant relationship between any of the independent variables and House or Senate vote. At the other extreme, the saturated model (VRPF) includes all four variables and all possible interactions among them. Identifying the most parsimonious model that also fits the data begins with a comparison of (V) and (VRPF) to determine whether vote is unrelated to all three independent variables—region, partisan affiliation, and past financial situation. The test involves subtracting the maximum-likelihood chi-square values (L^2s) and comparing the difference to the difference in degrees of freedom. Looking at the subsample of those voting in House elections, Table 2 shows that the difference in L^2 (264.30 — 0) is significant at the .001 level with 17 degrees of freedom (17 — 0), which means at least one of the independent variables must be included in the final model. We thus proceed to (VR)(VP)(VF), which includes the relationships between each independent variable and vote, controlling for the other independent variables. The question is whether (VR)(VP)(VF) is a better fit to the data than the simpler model (VR)(VP), which does not include past financial condition as an independent variable. It hypothesizes, in other words, that region and partisan identification independently affect the House vote, while individuals' evaluations of changes in their economic situations do not. Comparing (VR)(VP)(VF) to (VR)(VP) indicates that the addition of (VF) results in a substantial reduction in L^2 at the cost of only two extra degrees of freedom; past financial condition cannot be dropped from the model. Similarly, although of secondary importance, region must be included in the final model.

At this point, our model is (VR)(VP)(VF). The final question is whether a model incorporating interaction effects among the variables fits the data significantly better than (VR)(VP)(VF). Accordingly, we compare the independent effects model to the more complex models. Substantively, for example, (VPF) posits that the level of economic voting varies by partisan identification. In each instance, the small reduction in L^2 relative to the cost in degrees of freedom, combined with the *p*-value of (VR)(VP)(VF), leads to acceptance of the simpler model.[9] Applying the same procedure to the subsample of Senate voters, we choose

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[9] An investigator normally looks for large chi-squares and correspondingly low *p*-values. Log-linear analysis reverses this logic. The chi-square difference and the *p*-value tests can occasionally lead to different conclusions about the most appropriate model, in which case theoretical considerations must dictate the final choice.

We might also note here that the L^2-difference test involving the final and the no-effects models is analogous to R^2 in regression. The L^2 of (V), for example, is 264.30, while the L^2 of (VR)(VP)(VF), the final model, is 11.21. Taking the ratio

$$\frac{264.30 - 11.21}{264.30},$$

(VP) as the most parsimonious model that closely fits the data.

The appropriate ßs are in Table 3. As expected, there is a strong relationship between voters' partisan affiliations and their votes in both House and Senate elections. In addition, region is statistically significant among the House subsample: southerners are more likely than non-southerners to vote Democratic.[10] More important, in neither electoral context do voters' assessments of changes in their past financial well-being condition their choices of candidates. Among those voting for candidates for House seats, neither those who believe that their personal financial situations have improved nor those who think that conditions have worsened give any indication that their political choices are a function of their economic evaluations. And, of course, the economic variable is not even included in the final model for voters in Senate elections. Our first cut of the data leads to a conclusion similar to that drawn in earlier studies: economic experience and political decision making are unrelated among the mass public. This conclusion applies to House and Senate elections alike.

Table 3. Beta Effects for Best-Fitting Log-Linear Model Involving Past Financial Well-Being Among Voters in House and Senate Elections

Independent Variable	β Parameters in Model: House Elections	β Parameters in Model: Senate Elections
Region		
South	.412*	
Partisan Identification		
Democrat	1.452*	1.476*
Independent	−.118	−.186
Republican	−1.334*	−1.290*
Past Financial Well-Being		
Better	.160	
Same	−.330*	
Worse	.170	

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

$^*p \leqslant .05$

Our initial discovery does not rule out the existence of economic voting, for as we noted earlier, economic voting implies a relationship between voters' *expected* financial condition and their vote. Table 4 lists the set of hierarchical models in which expected financial well-being has been substituted for respondents' perceived change in their financial situations during the past year. In terms of the L^2-differences and the p-values, the simplest acceptable model among voters in House elections is (VR)(VP). Expected financial well-being is not included in the model. Among those voting for senator, on the other hand, an accurate representation of the relationships in the data is given by the model that posits independent effects of partisan identification and expected financial situation on political choice.

Table 5 reports the appropriate coefficients showing the strength and direction of the relationships for each subsample of voters. Partisan identification and region once again are important and separate determinants of the vote among individuals voting in House elections. While party affiliation also continues to condition the vote among the Senate subsample, more noteworthy is the fact that expectations about financial well-being during the upcoming year are significantly and strongly related to support for Senate candidates. Individuals who feel that their personal financial situations are likely to deteriorate are considerably more inclined to vote Republican than is expected under conditions of independence. Similarly, support for Democratic candidates is strong among Senate voters who anticipate an upward swing in their personal financial situations. Using individuals' *expectations* about their financial conditions, as the economic voting model requires, we find evidence that voters draw upon their personal economic circumstances to make midterm judgments.[11] But such judgments are

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we can interpret the final model as explaining approximately 95 percent of the "original unexplained variance." It is important to remember that this ratio and R^2 are not equivalent.

[10] In the ECTA program, tests of statistical significance make use of the standardized parameters, which, strictly speaking, apply only to the saturated model. These parameters define the lower bounds on the estimated effects of unsaturated models and thus are conservative measures of significance of association.

[11] In addition to region and party, we controlled for the effects of incumbency and evaluations of presidential performance. The incumbency control is particularly important given the Senate sampling problems. Having found that region has no independent effect on the Senate vote, we substituted each of these two controls for it and thus avoided the problem of numerous empty cells. In both instances, our conclusions remain intact, although the addition of incumbency reduces the relationship between expected financial well-being and Senate vote slightly.

Another potential control variable is candidate preference, as measured by the relative rankings of the incumbent and the challenger on the feeling thermometer. Mann and Wolfinger (1979, p. 22) have noted, how-

Table 4. Log-Linear Models for Vote, Region, Partisan Identification, and Expected Financial Well-Being Among Voters in House and Senate Elections

	Likelihood-Ratio Chi-Square		P-Value		
Model	House Elections	Senate Elections	House Elections	Senate Elections	Degrees of Freedom
(VRPF′)	0.00	0.00	–	–	0
(RPF′) (VRP) (VPF′)	8.79	4.73	.186	>.5	6
(RPF′) (VRP) (VF′)	11.54	5.68	.317	>.5	10
(RPF′) (VPF′) (VR)	9.28	9.63	.319	.292	8
(RPF′) (VPF′)	26.00	9.63	.002	.381	9
(RPF′) (VR) (VP) (VF′)	12.12	10.58	.437	>.5	12
(RPF′) (VR) (VP)	14.68	17.43	.401	.234	14
(RPF′) (VP) (VF′)	28.60	10.58	.008	>.5	13
(RPF′) (VP)	31.17	17.45	.008	.293	15
(RPF′) (V)	251.34	145.57	.000	.000	17

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

Key: V = Vote
R = Region
P = Partisan Identification
F′ = Expected Financial Well-Being

selective: they occur in Senate but not House elections. The context within which voting takes place apparently is an important determinant of whether the most fundamental of political acts functions as an expression of economic concern.

Employment Status and Choices of Candidates in House and Senate Elections

One of the central characteristics of the economic voting model is its short-term perspective. It predicts that voters' short-term expectations about their own economic circumstances will influence their judgments about congresssional candidates. We identified such behavior with regard to citizens' anticipated financial well-being, but again, only within the context of Senate elections.

Although it has generally gone unnoticed, there is one economic problem on which the utility of the model is inherently limited—unemployment among the electorate. While the short-term perspective of the model makes sense with regard to very general economic concerns, such as those we have been considering, the distinct and consistent positions of the two parties over time on this most

Table 5. Beta Effects for Best-Fitting Log-Linear Model Involving Expected Financial Well-Being among Voters in House and Senate Elections

	β Parameters in Model	
Independent Variable	House Elections	Senate Elections
Region		
South	.436*	
Partisan Identification		
Democrat	1.446*	1.470*
Independent	–.090	–.198
Republican	–1.356*	–1.272*
Expected Financial Well-Being		
Better		.426*
Same		–.044
Worse		–.382*

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

*$p \leqslant .05$

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ever, that "94 percent of all respondents who prefer the [House] incumbent vote for the incumbent; 88 percent of all respondents who prefer the challenger vote for the challenger." A similar relationship obtains among Senate candidates. These extremely strong relationships indicate, in accord with extant psychological research, that candidate preference and vote are, for all practical purposes, one and the same. To control for the former is to control for the latter, which is what we are trying to explain. Moreover, Hinckley (1979) has suggested that the thermometer ratings are best understood as summary measures that themselves are affected by various influences. Since our purpose is to estimate the *independent* effects of theoretically relevant influences, inclusion of candidate preference would only serve to contaminate our analysis.

debilitating of all economic problems portends a long-term perspective. Historically, the Democratic party has taken stronger positions on the unemployment problem than the Republican; the unemployed should be especially sensitive to these party differences when the president and Congress are faced with redressing *both* severe unemployment and severe inflation, as was true in 1978. Such sensitivity is reflected in the data. Among the currently unemployed who voted in 1978 (at least as ascertained by the CPS survey), 24.5 percent believe the Democratic party is more competent on unemployment than the Republican; only 9.1 percent feel the Republican party is more capable.[11] In essence, despite their current status and contrary to the prediction of the economic voting model, the unemployed should, on the whole, be inclined to vote Democratic because they anticipate that a Democratic administration will be more likely than a Republican administration to improve their personal situations in the future.

The 1978 CPS survey includes a large number of questions about the respondent's employment status. In the name of parsimony, we use a dichotomous variable that categorizes the respondent as currently employed or unemployed.

The data in Tables 6 and 7 bear out the prediction that the unemployed will vote disproportionately Democratic, *but once again the nexus between personal economics and political choice is limited to the context of Senate elections.* On the one hand, as is consistent with our earlier findings, voting in House elections is a function solely of partisan identification and region; employment status has no independent effect of its own. Within the context of Senate elections, on the other hand, the unemployed are considerably more likely to vote Democratic than is predicted on the basis of chance alone. The historical records of the two parties on unemployment seemingly are not lost on those most directly affected.

Like expected financial condition, then, employment status conditions the vote for candidates to the upper but not the lower chamber of Congress. While one set of economic judgments is based on a short-term perspective, the other is based on the accumulation of experience over time.

Economic Voting and Popular Control of Economic Policy

Sampling problems associated with the 1978 national survey recommend a cautious and restrained concluding statement. Nonetheless, there was, in 1978, a significant relationship between citizens' expected financial well-being and their votes in at least some (and we suspect most) Senate races. We also found a strong association between employment status and the Senate vote. For reasons not totally known but presumably having much to do with the incumbency phenomenon, on the other hand, there was no evidence of a link

Table 6. Log-Linear Models for Vote, Region, Partisan Identification, and Employment Status Among Voters in House and Senate Elections

Model	Likelihood-Ratio Chi-Square House Elections	Likelihood-Ratio Chi-Square Senate Elections	P-Value House Elections	P-Value Senate Elections	Degrees of Freedom
(VRPE)	0.00	0.00	–	–	0
(RPE) (VRP) (VPE)	2.45	0.84	.484	>.5	3
(RPE) (VRP) (VE)	4.33	1.04	>.5	>.5	5
(RPE) (VPE) (VR)	3.32	5.89	>.5	.317	5
(RPE) (VPE)	18.68	5.91	.005	.433	6
(RPE) (VR) (VP) (VE)	5.29	6.14	>.5	>.5	7
(RPE) (VR) (VP)	7.46	23.55	.488	.003	8
(RPE) (VP) (VE)	21.09	6.16	.007	>.5	8
(RPE) (VP)	23.09	23.57	.006	.005	9
(RPE) (V)	255.86	157.58	.000	.000	11

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

Key: V = Vote
R = Region
P = Partisan Identification
E = Employment Status

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[11] As expected, similar perceptions prevail among the employed.

Table 7. Beta Effects for Best-Fitting Log-Linear Model Involving Employment Status Among Voters in House and Senate Elections

	β Parameters in Model	
Independent Variable	House Elections	Senate Elections
Region		
South	.404*	
Partisan Identification		
Democrat	1.428*	1.452*
Independent	–.116	–.204
Republican	–1.312*	–1.248*
Employment Status		
Unemployed		.740*

Source: 1978 National Election Study, Center for Political Studies, University of Michigan.

$*p \leq .05$

between personal economic condition and political decision in House elections. We conclude that the electoral context makes a difference with regard to the presence or absence of economic voting. Indeed, only when the nature of the context is explicitly taken into account do the reported relationships take on full meaning.

As we noted earlier, the significance of our analysis extends beyond the conclusion that context matters to a fundamental democratic concern: citizen control and direction of national policy. From the perspective of popular control, voting on the basis of economic considerations is not only desirable but essential, for otherwise public sentiments for change are unlikely to be translated into public policy (cf. Asher and Weisberg, 1978). It might be argued, of course, that popular control of economic policy itself is not desirable. Keech (1980, p. 366), for example, on the basis of his innovative work, concludes that "the [macroeconomic] policy results of a fair and open electoral process are not necessarily optimal."

Assuming, nonetheless, that popular control of public policy remains a central tenet of contemporary democratic theory, what are the implications of our results? The temptation is to suggest that popular control exists, but only within the context of Senate elections. We refrain from this conclusion, however, out of a recognition that economic voting is only a necessary condition of citizen influence on government performance. Because of formidable constraints inherent in the political system, there is no guarantee that voters will bring about economic change even when they act on their grievances.

Consider, as a starting point, the obvious fact that individual voters can control policy only through collective action. But if *individuals* are *collectively* to control economic performance, there must be widespread consensus among them. Such consensus is precisely what Key (1964, pp. 562-68) had in mind when he discussed popular control in terms of "broad sweeps of sentiment." At least as was ascertained by the 1978 CPS survey, this condition did not obtain in that year, for citizens were nearly evenly split in their assessments of future financial conditions. Votes cast on economic grounds undoubtedly neutralized each other in many instances.

Let us assume, nonetheless, that a very clear consensus existed in 1978. Even then the electorate was able to vote for only one-third of the total Senate membership. Because of the institutional arrangements established by the Founding Fathers, voters cannot simultaneously make judgments on all the men and women who determine the direction of economic policy. Although they admittedly can use their votes to send a "message," election returns are not readily interpretable as policy directives.

A more illuminating if not more significant constraint has to do with the options available to the voters. The unemployed represent a case in point. Unemployment was high in 1978 and at the time of the election there was no reason to believe that it would lessen. Yet the party widely perceived as having the best track record on unemployment was in office and had been there for some time. The unemployed, in 1978, could vote for the party that historically had been sensitive to their needs but was now floundering badly; they could opt for a change and vote contrary to past experience; or they could abstain. The options available to the unemployed to improve their lot were, needless to say, limited. Although they remained overwhelmingly loyal to the Democratic party, they presumably did so without great enthusiasm.[13]

More generally, the evidence of the 1970s points to the national government's growing inability to carry out effective macroeconomic policy. Traditionally the conflict has been over the relative priority of inflation and unemployment. One could be reduced, even though at the expense of the other. But the last two administrations—one

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[13] Even if a viable and meaningful option had been available, the unemployed are, as a group, sufficiently small and dispersed among individual congressional districts and states to preclude them from directly determining who will and will not hold office. The unemployed are largely dependent on the employed to express a concern for the unemployment problem.

Republican, the other Democratic—have been unable to control either. It is beyond the scope of this article to try to explain these trends, although factors outside the United States political system, such as Arab oil prices, and the apparent deterioration of the party system in Congress suggest themselves as likely candidates. The point is that when the options available to government are reduced, so are the opportunities of the electorate to effect meaningful economic change.

Given these constraints, it is not too surprising that individual voters support House incumbents who have already shown that they can effectively deliver the goods upon individual request. Indeed, it may be the *most* rational choice for the individual voter who has benefited from personalized constituent services in the past—and expects to benefit in the future. The question may be less why citizens do *not* relate their economic concerns to their votes in House elections than why they *do* in Senate elections. If Senators should follow the path of House members and become increasingly effective in activities that directly benefit individual constituents, even this question may become moot. The paradox, of course, is that as individual constituents increasingly hold individual representatives responsible for provision of particularized services, collective influence on the direction of government policy making diminishes.

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Policy-Oriented Voting in Response to Economic Issues

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This study explores the hypothesis that voting in response to economic problems is policy-oriented: voters concerned about unemployment are predicted to give greater support to Democratic candidates, while those concerned about inflation are predicted to vote more Republican. In light of evidence from previous research, this study investigates the electoral effects of inflation and unemployment as (1) problems directly experienced by the individual, and (2) problems deemed serious for the nation as a whole. Support is strongest for the unemployment side of the hypothesis. Voters personally affected by unemployment gave a modest boost to Democratic candidates in virtually every election. And in years of high unemployment the large percentage of voters who felt it was a serious national problem voted heavily Democratic as well. This study concludes by discussing the important implications these findings have for our understanding of how economic conditions influence voting behavior in American national elections.

Recently several studies have appeared concerning the impact of economic concerns upon voting. Largely motivated by a desire to find patterns of individual behavior consistent with the strong aggregate relationships which Kramer (1971) and others have uncovered, these studies focused on an *incumbency-oriented* hypothesis, i.e., the theory that voters support or oppose the incumbent president and candidates of his party in response to fluctuations in economic conditions.

Most of these survey analyses have found clear evidence of incumbency-oriented voting. Interestingly, the major source of support for the hypothesis appears not to be individuals' assessments of their own personal financial situation. Such perceptions exerted only weak, scattered effects upon voting in congressional, senatorial, and gubernatorial elections, and more consistent but still quite modest effects in presidential races (Ben-Gera Logan, 1977; Fiorina, 1978; Klorman, 1978). Rather, it is assessments of the *nation's* economy which appear to have the greater impact. Kinder and Kiewiet (1979) found that voters who felt national business conditions had recently worsened were consistently more likely to vote against congressional candidates of the president's party than voters who believed conditions had improved. The evidence was similarly supportive of this proposition in senatorial and presidential races (Ben-Gera Logan, 1977; Fiorina, 1979).

The concentration of work in this field upon the single hypothesis of incumbency-oriented voting, however, implies some serious limitations in our knowledge of how economic concerns influence voting decisions. This study addresses at least one major deficiency in this literature by investigating the hypothesis, suggested by Stigler (1973) and others, that voting in response to economic problems is *policy-oriented,* especially with regard to inflation and unemployment. Given the different policy priorities of the two major parties (Sundquist, 1968; Okun, 1973; Hibbs, 1977; Tufte, 1978), a policy-oriented hypothesis predicts that, everything else being equal, voters concerned about unemployment will tend to support Democratic candidates, while voters more concerned about inflation will opt for the Republicans.

To characterize such voting as "policy-oriented" is not to imply that voters need a sophisticated understanding of fiscal or monetary policy instruments. Neither must they believe anything in particular about the empirical relationship between inflation and unemployment, nor recognize that full employment and price stability pose conflicting goals. Rather, it requires only that they (1) see either inflation or unemployment as a serious problem and want to see it alleviated, and (2) perceive differences between the parties in the amount of effort and/or skill they apply in combating that problem.

One other point deserves comment before we proceed. This hypothesis seems to imply, paradoxically, that when in office the Republicans could bolster their reelection prospects by running up a high inflation rate, while the Democrats have the same perverse incentives with regard to unemployment. Such a "strategy," however, would almost certainly be counterproductive. A complete flip-flop on macroeconomic policy priorities would betray the party's key constituent groups and alienate some of its most dependable supporters. The resulting uncertainty as to what the party could be expected to do in office would also scare off many others (Downs, 1957). And as Okun (1973) points out, a party's long-term favorable image vis-à-vis unemployment or inflation would surely be eroded by such policy failures.[1]

The following analyses thus examine the hypothesis that voting in response to inflation and unemployment is policy-oriented. In light of the evidence on incumbency-oriented voting, this study will investigate the impact of (1) inflation and unemployment as personally experienced economic problems, and (2) perceptions of inflation and unemployment as serious national problems. The survey data used come from the biennial CPS American National Election Studies.[2]

The Effects of Concern over Inflation and Unemployment upon Voting in National Elections

Given their focus on incumbency-oriented voting, studies in this area have relied mainly upon the same measures of perceived trends in one's own financial situation. A couple of previous analyses, however, did look at personal difficulties due to unemployment (Kinder and Kiewiet, 1979; Fiorina, 1980). Kinder's and Kiewiet's analysis used a dummy variable which registered whether or not the respondent's head of household had been out of work at some time in the last year or two. This measure did no better than the personal finances item in predicting votes for or against the incumbent party's candidates. When their results are examined from the perspective of policy-oriented voting, however, a striking pattern emerges. After making a few desirable changes in their estimation procedures, I present these results in Table 1. The top numbers in each entry are the probit estimates, the numbers in parentheses beneath them are standard errors. As in their original analysis, the personal (family) finances item was also specified (the "better off," "same," and "worse off" categories taking on values of 1, 3, and 5, respectively) as were party identification terms.[3] Estimates of the latter, of course, were always quite large and statistically significant. This has no direct bearing on the analysis here, however, and so for the sake of conciseness I do not report these estimates. Reported votes for Democratic candidates are represented by 0, and reported Republican votes by 1, so positive signs are in a pro-Republican direction.

As previous studies had shown, effects associated with personal financial assessments were generally in keeping with incumbency-oriented voting. Also as before, effects were weak and erratic in congressional elections, but somewhat stronger in presidential elections. Signs of the estimates were in the correct (incumbency-oriented) direction in all six presidential elections, and four of the six were statistically significant. Only eight of the 12 estimates for congressional races were consistent with incumbency-oriented voting, and only three were statistically significant (the significant 1966 estimate was in the wrong direction).

None of this is very new or exciting. What is striking about Table 1 is that the sign of the head of household's unemployment term is in a negative and thus pro-Democratic direction in 18 of the 19 elections studied. The only exception is the 1968 presidential race, an election in which George Wallace made substantial inroads into the Democratic party's traditional blue-collar constituency. Whether or not a Democrat sat in the White House, respondents from households whose head had been out of work consistently gave more support to candidates of the Democratic party.

As with the personal finances measure, estimates for the unemployment term were stronger in presidential elections than in congressional ones. Weighting each estimate by the inverse of its standard error, the mean estimates were —.383 and —.235, respectively. This makes sense, given the lower salience on congressional races (Stokes and Miller, 1962), and that the president is probably held more directly accountable for economic conditions than his party's House candidates. But the probit estimates varied widely in size; in six elections they exceed .5, but in four others they are less than .1. And only five of the nineteen attain convention levels of statistical significance. True, in 1956 and 1966 the unemployment measure reflects current status only, and the low N (in 1956 only 15 respondents reported that their head of household was currently unemployed) make statistical significance difficult to achieve. But low N is usually not the problem; an average of 40 voters reported their head of household had been out of work in the previous six months, and when the previous year is considered the average is 108. So while voters from families which have been affected by unemployment appear to give consistently greater support to Democratic candidates, the magnitude of this support is only occasionally impressive.

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[1] To be sure, the previous Democratic administration would seem to have adopted this strategy. President Carter fully supported the Federal Reserve's credit-tightening moves which helped trigger the 1980 recession. The argument here, though, is not that the Democrats care only about unemployment and pay no heed to inflation, while the Republicans are prepared to accept any rate of unemployment for the sake of price stability. It is, rather, that the parties' *relative* priorities differ, and this point seems secure. Even though Carter may have opted for typically Republican policies, it is hard to believe Reagan will be less zealous than Carter in the battle against inflation, or more troubled by unemployment.

[2] These data were made available by the Inter-University Consortium for Political and Social Research. Neither the original collectors of the data nor the Consortium bear any responsibility for the analysis or interpretations presented here.

[3] In the years in which panel data were available (1958, 1960, 1974, and 1976) dummy variables registering the respondents' vote in the previous election were used. In other years partisanship was specified by a pair of nominal party identification dummies. Results were virtually identical, however, when the nominal party identification dummies were substituted for the previous vote terms in the four panel years.

There are obvious shortcomings in this analysis. Only the unemployment side of the hypothesis has been addressed, and only at the personal level. These deficiencies will be remedied by (1) analyzing direct indicators of personal difficulties with inflation as well as unemployment, and by (2) investigating the effects of perceptions of unemployment or inflation as serious national problems.

Table 1. The Effect of Head of Household's Unemployment Experience and Trend in Family Finances on Voting in Congressional and Presidential Elections, 1956-1978

	Presidential Elections		Congressional Elections	
Year	Head's Unemployment Experience[a]	Personal Finances	Head's Unemployment Experience[a]	Personal Finances
1956	–.097 (.513)	–.134** (.031)	–.659 (.597)	–.087** (.034)
1958	–	–	–.413* (.243)	–.103** (.036)
1960	–.659** (.241)	–.046 (.030)	–.335 (.274)	–.019 (.033)
1962	–	–	–.244 (.230)	.037 (.042)
1964	–.579 (.355)	.040 (.033)	–.148 (.314)	–.014 (.035)
1966	–	–	–.630 (.560)	–.077* (.036)
1968[b]	.130 (.305)	.134** (.038)	–.411 (.308)	.037 (.048)
1970	–	–	–.211 (.308)	–.008 (.048)
1972	–.523** (.150)	–.078* (.037)	–.395* (.179)	–.022 (.041)
1974	–	–	–.041 (.155)	.032 (.030)
1976	–.210 (.131)	–.081** (.029)	–.039 (.157)	–.131** (.034)
1978	–	–	–.093 (.132)	–.007 (.026)

Source: American Panel Study, 1956, 1958, 1960; American National Election Studies, 1962-1970; American National Election Series, 1972, 1974, 1976; American National Election Study, 1978; Center for Political Studies, University of Michigan.

[a] In all elections between 1958 and 1964, as well as in 1968 and 1970, this variable indicated whether or not the respondent's head of household had been unemployed in the previous six months. From 1972 on the time frame was the previous year, and in 1956 and 1966 it could only be ascertained whether or not the head was currently out of work.

[b] Wallace voters were excluded from the presidential election analysis in 1968.

*$p < .05$
**$p < .01$

Personal Economic Problems. What is needed in order to conduct a more compelling test of the policy-oriented hypothesis at the personal level is a means of isolating those voters who find inflation or unemployment salient and troublesome. In three of the CPS election studies—1972, 1974, and 1976—this could be done on the basis of answers to the following questions:

> Let's change the subject for a moment. We like to have people tell us what sorts of problems they have to deal with in their daily lives. Can you tell me what some of the problems are that you face these days in your life. . . . Anything else?

Although these questions did not specifically refer to economic problems, well over half the problems mentioned were economic in nature.

Unfortunately, the coding categories developed by the CPS for these items were, for present purposes, not appropriate. It was therefore necessary to recode responses as they appeared on the original protocols.[4] References to unemployment and inflation were naturally sorted out, but other categories were derived from the verbatim interview data as well. The coding scheme which was developed, along with the marginal frequencies from all three years, is presented in Table 2. What are reported are the respondents' most important personal *economic* problems. If a respondent mentioned some non-economic problem as most important, e.g., poor health, but also mentioned an economic problem, the economic problem was the one which was coded here.

As Table 2 shows, respondents cited *inflation* more frequently than any other economic problem. This category includes all references to high or rising prices, either in general or for specific commodities. Many economists, of course, express puzzlement that anyone should be concerned about inflation in and of itself. What should worry people is the failure of pay increases to match price increases—a declining real income—and not the nominal price level. But as Table 2 indicates, between 5 and 7 percent of the respondents did refer explicitly to a *declining real income.* Responses subsumed under this category included failure of wages to keep up with price increases, living on a fixed income, declining purchasing power, failing to maintain the standard of living one is accustomed to, or having great difficulty in doing so.

The *unemployment* category is broadly defined. It contains all respondents who felt their worst economic problem was that they (or people close to them) were laid off, unemployed, worried by the threat of unemployment, or underemployed. Despite the breadth of this definition, only between 3.4 percent and 4.4 percent fell into this category. Perhaps, as some economists and politicians claim, unemployment compensation and other benefits have substantially eased the burden of joblessness for many people. Or it might be argued that many respondents felt that references to unemployment would reflect badly on themselves, and thus chose to name some other problem. This seems very unlikely, however, given that they showed no reluctance to admit that they (or family members) were out of work in response to other questions.

Table 2. Respondents' Most Important Personal Economic Problems (Percent)

	1972	1974	1976
Inflation	14.4	30.2	22.2
Declining Real Income	4.8	7.1	5.9
Unemployment	3.4	4.4	4.0
Taxes	5.5	1.3	4.1
General or Miscellaneous Economic Problems	18.6	19.2	19.8
Non-Economic Problems	32.5	24.4	26.8
No Problem Mentioned	20.8	13.4	17.2
N[a]	1109	2523	2415

Source: American National Election Series, 1972, 1974, 1976. Center for Political Studies, University of Michigan.

[a]Based on the 1972, 1974, and 1976 cross-sectional weighted samples of all respondents. No important turnout effects were present, however, so the percentages reported here are virtually identical when voters only are considered. Weights are *not* used in any of the probit analysis.

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[4] One of the major problems with the 1976 code was that complaints about unemployment were lumped together with all other job-related references; only about half of the references in the employment-related category concerned unemployment. Many of the 1972-74 codes contained a wide variety of responses. One category, for example, contained references to inflation, taxes, governmental waste, and the cost of college. These responses obviously need to be disaggregated.

Similarly, few respondents referred to *taxes* as their worst personal economic problem. A residual category, *general or miscellaneous economic problems,* was largely composed of vague references to such things as "bills," "finances," or "money problems." As Table 2 shows, the percentage of respondents falling into this category was remarkably stable—around 20 percent each year. In fact, the size of most categories remained quite stable from year to year—only the percentage citing inflation fluctuated much. Changes in the size of that category, though, clearly reflected the actual rates of inflation which obtained in 1972, 1974, and 1976.

At this point, it should be noted that personal problems due to inflation and unemployment differ in two fundamental ways. First, the serious objective costs of unemployment are reflected in the fact that respondents who cited it were far more likely than average to report that their financial situation had worsened. Respondents in the inflation category, on the other hand,, were not more likely than average to report a worsened financial situation. This is quite in keeping with economic studies reviewed by Hibbs (1979) which indicate that, at least in the short run, the objective income, wealth, and distributional effects of inflation are small. Hibbs is almost certainly correct in concluding that "less tangible subjective and psychological factors are probably more important than objective costs" in explaining public aversion to inflation (p. 712).

Secondly, these data reflect the fact that unemployment is concentrated disproportionately among lower-income and working class groups, while the less tangible costs of inflation are spread pretty evenly across society. In 1976 black respondents were more than twice as likely as whites to cite unemployment as their most serious economic problem. Among occupational categories, the percentages ranged from only 1.6 percent of managerial-administrative personnel to 14.0 percent of non-farm laborers. The propensity of respondents to cite inflation as their most serious economic problem, however, was unrelated to a host of background socioeconomic characteristics, including income, age, sex, race, and union membership. In short, there are major differences between inflation and unemployment in the nature of the costs they entail and how these costs are distributed.[5]

Perceived National Economic Problems. Any comparison between the personal-level and national-level versions of the policy-oriented hypothesis should be a fair one. A desideratum for the following analysis, therefore, is to develop measures of concern with national economic problems which are as similar as possible to the measures of personal economic problems. Fortunately, almost all CPS Election Studies since 1958 have contained questions suitable for present purposes. Like the personal problems questions, they were all open-ended, imposed a minimum of constraint on responses, and did not refer to any parties or candidates.

The various questions used were as follows:

- What do you think are the most important problems facing this *country*? (1972, 1974, 1976, 1978).
- What do you personally feel are the most important problems which the government in Washington should try to take care of? (1966, 1968, 1970).
- What would you personally feel are the most important problems the government should try to take care of when the new President and Congress take office in January? (1960, 1964).
- Now, how about problems here at home inside the United States in the past year or so. . . . Would you say that things in general have been going along better than they were a year ago, not as well as before, or have they stayed about the same? How is that? (1958).

The coding scheme derived from the available CPS categories and marginal frequencies for each year are presented in Table 3. As before, it is the most important *economic* problems respondents cited which are reported; if a non-economic problem (e.g., Vietnam) was deemed most important, but an economic problem was also mentioned, the economic problem was the one which was coded.[6]

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[5] Parenthetically, these data indicate that it was useful to differentiate respondents who cited a declining real income from those who simply cited inflation. The former were considerably more likely to report being worse off financially, and such references came considerably more frequently from low-income groups and retired persons.

[6] In all studies except 1958, respondents were subsequently asked which of the problems they had mentioned they considered most important. Problem reports could thus be ordered according to perceived importance, and the highest ranking economic problem was included in Table 3. The procedure followed in the 1958 study was more complicated. After indicating whether they believed "problems here at home in the United States" had got better in the past year, got worse, or stayed the same, respondents were asked to report the particular problems they had in mind. Only the reported problems of those who believed things had stayed the same or had worsened were coded, but about 85 percent of the sample fell into these two categories. As Table 3 indicates, inflation and unemployment were the only two economic problems which could be extracted from the CPS codes for that year. The "no problem men-

Most categories require little explanation. The *inflation* category includes all those who named high or rising prices as the nation's most serious economic problem. Included under *unemployment* are respondents who felt the government should create more jobs, provide job retraining, or grant aid to depressed areas; most, though, simply cited unemployment. As Table 3 clearly shows, the number of respondents who named either of these two economic problems as most important closely tracked the actual inflation and unemployment rates in these years. The unemployment figures peaked in 1958 and 1976, inflation in 1974 and 1978. It should be noted that in the three years in which comparisons are possible (1972, 1974, 1976), the number of respondents who cited inflation or unemployment at the national level was always larger than the number who cited them as personally troublesome. The potential impact of voters' perceptions of national-level issues upon elections is thus greater, especially with regard to unemployment. Only a small percentage ever cited unemployment as a personal problem, but in 1958 and 1976 it was cited more frequently as a problem affecting the nation as a whole than any other problem.

As at the personal level, few respondents felt *taxes* were the most important national economic problem. Under *more government programs* were respondents who believed that the federal government should do more to alleviate social ills. Most of them advocated new or enlarged federal programs in particular areas, e.g., housing, transportation, health care, poverty programs, or social security. The category also included those who felt the federal government should increase its spending to stimulate the economy, or exert more control over private business. Individuals in the next category, however, believed that a large and growing public sector was precisely what was wrong with this country. A variety of responses fell under the rubric of *less government spending* —the federal government's budget deficits, waste and inefficiency, creeping socialism, or too much government interference into private enterprise. As Table 3 shows, the percentage of respondents

Table 3. Respondents' Perceptions of the Nation's Most Important Economic Problems, 1958-1978 (Percent)

	1958	1960	1964	1966	1968	1970	1972	1974	1976	1978
Inflation	6.4	3.7	1.2	15.9	2.6	14.2	20.0	50.2	30.3	52.2
Unemployment	33.9	11.6	6.9	2.6	2.3	7.9	6.3	6.6	32.1	4.6
Taxes	–	4.7	4.3	6.4	3.7	6.1	5.5	0.3	1.6	2.5
More Government Programs	–	20.3	27.3	19.8	16.1	26.1	10.9	1.7	4.2	3.3
Less Government Spending	–	3.5	3.6	7.2	2.5	1.9	3.3	1.0	2.3	2.6
General Economic Problems	–	2.9	0.6	1.9	0.8	4.9	8.0	26.5	14.1	6.3
Non-Economic Problem	18.2	42.1	37.9	38.5	69.4	34.6	42.7	10.8	9.3	23.8
No Problem Mentioned	41.5[a]	11.2	18.2	7.7	2.6	4.3	3.3	2.9	6.1	4.7
N	1514	1514	1571	1291	1557	1580	1109	1624	1217	2304

Source: American Panel Study, 1956, 1958, 1960; American National Election Studies, 1964-1970; American National Election Series, 1972, 1974, 1976; American National Election Study, 1978; Center for Political Studies, University of Michigan.

[a] The "no problem mentioned" category in 1958 is not comparable to the same category in the other years. See footnote 6 for details. Column totals may not add exactly to 100 percent because of rounding error.

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tioned" category is unusually large in 1958 because responses relating to defense and foreign affairs were inappropriate, and because it includes the 15 percent who felt the U.S. domestic situation had been improving.

in the more government programs and less government spending categories dropped off considerably after 1970. But given the traditional differences between the two parties on the proper size and scope of the public sector (Tufte, 1978), it is probably important for the following analysis that these categorical variables be specified.

Finally, most respondents included in the next category, *general economic problems,* referred simply to "the economy," although a few cited such problems as interest rates or a bearish stock market. This category was quite small during the prosperous sixties, but by 1974 over a quarter of the respondents were included in it. As was also the case at the personal level, no obvious policy-oriented hypotheses can be made about respondents who cited general or miscellaneous economic problems. It seems reasonable to posit, though, that such voters might tend to vote against the incumbent president and his party's candidates.

Non-economic problems, of course, include everything else: the Vietnam War, national defense, crime, and a host of other things. Not surprisingly, this catch-all category was largest in the tumultuous, war-torn, but prosperous year of 1968, smallest in the peaceful but economically troubled period beginning in 1974.

The Nature of National Economic Problem Perceptions. An intriguing finding in Kinder's and Kiewiet's study (1979) was that individuals' assessments of their personal financial situation were only weakly related to their perceptions of national business conditions. However, probit analyses of references to unemployment and inflation at the national level indicated that individuals' personal economic problems clearly influenced these perceptions. (For the sake of brevity only a summary of these analyses can be reported—a report on estimation procedures and results is available from the author upon request.) Respondents who named unemployment as a problem at the personal level were much more likely to cite it as a problem at the national level. The same was true for inflation.

When group differences and party identification were considered, though, the same pattern present at the personal level reappeared. Blacks, union members, and low-income respondents were more likely than average to name unemployment the nation's most serious economic problem. Democrats were also more likely, but differences between partisan groups were generally smaller than they were between social, economic, and racial groups. In contrast, the percentage of respondents naming inflation as the most serious national economic problem was nearly constant across all major socioeconomic and partisan groups.

To be sure, the strength of the personal-national associations should not be overstated. Despite the greater propensity of respondents who referred to inflation and unemployment as personal problems also to cite them at the national level, the large majority of those who named them at the national level had not referred to them as personal problems. Furthermore, the impact of party identification upon people's perceptions of national economic problems was quite small in comparison to the overall variation in perceptions of inflation and unemployment from year to year. In short, perceptions of national economic problems, though affected to some degree by the respondents' personal economic experiences, socioeconomic background, and party affiliations, closely reflect actual conditions in the nation's economy.

A couple of other matters must be considered before the electoral effects of concern over inflation and unemployment can be estimated. First, although perceptions of national problems are evidently not very susceptible to partisan coloration, such perceptions might well produce changes in partisanship. If so, a cross-sectional analysis which specified partisanship might underestimate the effects of perceived national problems upon voting. Fortunately, this threat proved not to be serious. Cross-tabular analyses revealed that change in respondents' party identification from 1974 to 1976 was only weakly related to perceptions of national economic problems. Furthermore, consistent with Brody's findings (1977), what little systematic change there was occurred in the strength components—respondents perceiving inflation to be the nation's worse problem tended to become more strongly identified with the Republicans, while unemployment tended to strengthen the identification of those who had already been Democrats in 1974. In specifying partisan predispositions, therefore, we will use these procedures in the following analyses: when panel data are available, the respondent's vote in the previous election will be used ("previous election" means the previous congressional election when congressional elections are analyzed, but the previous presidential election when presidential elections were analyzed—in 1976, for example, the previous election considered in the congressional equation was 1974, but for the 1976 presidential race the respondents' reported vote for president in 1972 was specified). When only cross-sectional data are available, the partisanship indicators will incorporate the directional component only.

Finally, both the personal and national level indicators of concern over inflation and unemployment reflect only the presence of these problems, and not their absence. Now it may well be that the parties benefit electorally from dealing successfully with these problems. Success of this nature, however, would be indicated by the percentage of respondents citing inflation or unemployment approaching zero, and any electoral effects could not be detected by an analysis based upon these measures. Estimation will thus be confined to those years in which inflation or unemployment was bad enough to prompt a significant number of respondents (over 10 percent) to cite one of these problems at either the personal or national level. These years are 1958, 1960, and 1970 through 1978.

Estimation. The following analysis is a simple one. The probit model to be estimated is of the form:

$$V^{P,C} = f(\beta_0 + \beta_1 R + \beta_2 D + \beta_j PE_j + \beta_k NE_k + u)$$

where $V^{P,C}$ = respondent's reported vote for president or congressman, taking on the value of 0 if Democratic, 1 if Republican.

β_0 = a constant term.

R = a dummy variable which, when only cross-sectional data are available (1966, 1970, 1972, 1978), takes on the value of 1 if the respondent identifies with Republican party (as either a strong or weak identifier, or as an Independent leaning Republican), 0 otherwise. When panel data are available (1958, 1960, 1974, and 1976), the respondent's vote in the previous election is used. This variable then takes on the value of 1 if the vote was Republican, 0 otherwise.

D = the same as above, except it registers Democratic identifiers or respondents who voted Democratic in the previous elections. A reference group is thus composed of respondents who, in years in which cross-sectional data are used, are Independents, or, when panel data are used, had not voted in the previous election.

PE_j = in 1972, 1974, and 1976 a battery of dummy variables, one for each of the categories of personal economic problems which were coded. The reference groups are composed of those respondents who either reported no economic problems or no problems whatsoever. In other years the same personal finances and head of household's unemployment terms used previously are included.

NE_k = a battery of dummy variables, one for each of the categories of perceived national economic problems which were coded. As above, the reference groups are composed of respondents who did not cite an economic problem.

u = a randomly distributed error term.

Results are presented in Tables 4 and 5. As before, the top numbers in each entry are probit estimates, the numbers in parentheses below them are standard errors. Democratic votes were coded 0 and Republican votes 1, so positive signs are pro-Republican.

At the personal level, results were much the same as in the previous analysis. Coefficients of the personal finances and head of household unemployment terms were virtually unchanged; thus the head of household unemployment variable continued to have a consistent but usually small pro-Democratic impact upon voting. Substituting the personal economic problem items for these measures provides some additional support for the policy-oriented hypothesis. As before, the most impressive figures are for the unemployment variable—signs of all five estimates are pro-Democratic in direction, and most estimates are fairly large. True, none is statistically significant, but the small size of the categories would make conventional significance levels difficult to achieve.

The inflation side of the hypothesis, however, fares poorly at the personal level. True, every estimate of the inflation term is more positive and thus more pro-Republican than the corresponding unemployment term. But this is largely due to the consistent, pro-Democratic direction of the unemployment terms; most estimates for the inflation terms did not differ noticeably from those of the neutral, nondescript reference groups. The one bright spot is the 1972 presidential election, where the sign was in the correct direction and statistically significant. Apparently the only Democratic candidate punished by voters who felt personally injured by inflation was the unfortunate George McGovern. There is little to report concerning the other personal economic problems included in the analysis. In general, estimates for the effect of a declining real income fell in between the inflation and unemployment terms, with respondents voting more Democratic than the former but more Republican than the latter. Estimates of the taxes term were erratic in direction, while those for the general economic problems category hovered around zero.

Turning to perceptions of *national* economic problems, the unemployment side of the policy-oriented hypothesis again receives strong empirical backing. Voters who named unemployment the most serious national economic problem gave considerably more support to Democratic presidential candidates in 1960 and 1976 and to that party's congressional candidates in 1958. To be sure, the sizes of the probit estimates in these three elections were not overwhelming, and were generally smaller than those for the personal unemployment terms. This means that for any one individual, personally experienced unemployment

Table 4. The Effect of Personal and Perceived National Economic Problems on Voting in Presidential Elections, 1960-1976

	Presidential Elections		
	1960	1972	1976
Intercept	.207	.567	–.131
Democrat[a]	–1.08**	–.826**	–.715**
	(.112)	(.179)	(.149)
Republican[a]	.577**	.840**	.731**
	(.092)	(.198)	(.128)
Personal Economic Problems			
Family Finances	–.049*	–	–
	(.030)		
Head of Household's Unemployment	–.638**	–	–
	(.244)		
Inflation	–	.273*	–.037
		(.166)	(.116)
Declining Real Income	–	–.087	–.152
		(.233)	(.208)
Unemployment	–	–.346	–.338
		(.296)	(.258)
Taxes	–	.070	–.047
		(.234)	(.233)
General Economic Problems	–	.067	–.088
		(.145)	(.123)
National Economic Problems			
Inflation	.395*	.040	.104
	(.231)	(.143)	(.148)
Unemployment	–.257*	.022	–.361**
	(.138)	(.220)	(.148)
Taxes	.010	.360	***
	(.204)	(.267)	
More Government Programs	–.065	–.072	.096
	(.103)	(.176)	(.265)
Less Government Spending	–.160	.198	.269
	(.206)	(.308)	(.329)
General Economic Problems	.010	–.417*	.029
	(.222)	(.198)	(.171)
N	1149	760	923

Source: American Panel Study, 1956, 1958, 1960; American National Election Studies, 1966, 1970; American National Election Series, 1972, 1974, 1976; American National Election Study, 1978; Center for Political Studies, University of Michigan.

[a] These terms are nominal party identification in 1966, 1970, 1972, and 1978. In other years they indicate the respondents' vote in the previous election.

*$p < .05$

**$p < .01$

***Insufficient response

probably exerts a slightly larger influence upon voting than does the perception of unemployment as a serious national problem. But as I have pointed out earlier, the number of voters who perceived unemployment to be nationally troublesome in these years was a large multiple of the number for whom it was personally troublesome. The national-level measure included over eight times as many voters as the personal level measure in 1976, over three times as many in 1960, and

Table 5. The Effect of Personal and Perceived National Economic Problems on Voting in Congressional Elections, 1958-1978

	Congressional Elections							
	1958	1960	1966	1970	1972	1974	1976	1978
Intercept	.052	–.149	.053	–.077	1.02	–.498	–.327	–.159
Democrat[a]	–.964**	–.759**	–.892**	–.831**	–1.00**	–.573**	–.293**	–.604**
	(.133)	(.105)	(.182)	(.181)	(.188)	(.129)	(.125)	(.140)
Republican[a]	.998**	1.27**	.986**	1.09**	.824**	1.12**	1.17**	.682**
	(.119)	(.120)	(.188)	(.188)	(.189)	(.129)	(.139)	(.144)
Personal Economic Problems								
Family Finances	–.105**	–.021	–.077**	–.008	–	–	–	–.008
	(.036)	(.033)	(.037)	(.049)				(.026)
Head of Household's Unemployment	–.387	–.344	–.720	–.193	–	–	–	–.094
	(.245)	(.275)	(.555)	(.222)				(.133)
Inflation	–	–	–	–	–.024	–.037	–.244*	–
					(.175)	(.116)	(.136)	
Declining Real Income	–	–	–	–	–.187	–.234	–.137	–
					(.296)	(.176)	(.229	
Unemployment	–	–	–	–	–.537	–.214	–.409	–
					(.412)	(.287)	(.304)	
Taxes	–	–	–	–	–.389	***	.235	–
					(.250)		(.256)	
General Economic Problems	–	–	–	–	–.094	–.104	–.010	–
					(.159)	(.142)	(.147)	
National Economic Problems								
Inflation	–.035	.176	.325*	.216	–.277	.037	.103	.011
	(.211)	(.269)	(.165)	(.171)	(.163)	(.167)	(.175)	(.108)
Unemployment	–.289**	.015	.694*	–.162	.074	.044	–.218	–.106
	(.110)	(.151)	(.317)	(.222)	(.246)	(.237)	(.169)	(.251)
Taxes	–	.101	.053	–.090	–.123	***	***	.179
		(.241)	(.242)	(.272)	(.263)			(.260)
More Government Programs	–	.194*	–.032	.031	–.135	***	.943**	.279
		(.117)	(.151)	(.144)	(.180)		(.307)	(.276)
Less Government Spending	–	–.057	–.160	.425	–.141	***	–.115	.110
		(.227)	(.198)	(.377)	(.321)		(.354)	(.233)
General Economic	–	.173	***	–.061	–.217	.162	.146	.047
		(.236)		(.227)	(.217)	(.178)	(.196)	(.167)
N	825	987	677	683	692	863	689	1009

Source: American Panel Study, 1956, 1958, 1960; American National Election Studies, 1966, 1970; American National Election Series, 1972, 1974, 1976; American National Election Study, 1978; Center for Political Studies, University of Michigan.

[a] These terms are nominal party identification in 1966, 1970, 1972, and 1978. In other years they indicate the respondents' vote in the previous election.

*$p < .05$

**$p < .01$

***Insufficient response

over six times as many in 1958. Taking this into account, it is clear that in elections which follow severe recessions (1958 and 1976) or periods of slow growth (1960), the electoral impact of unemployment derives mainly from voters who see it as a pressing national problem, and not from those who have been personally affected.

As to the personal level, though, the inflation side of the hypothesis does not fare as well. The estimate in the 1960 presidential election was in the correct direction and statistically significant. And support for the hypothesis was quite strong in 1966, when the large percentage (16 percent) of voters who cited inflation were considerably more likely to vote Republican. It seems hard to believe that the 2.8 percent rise in consumer prices that year triggered such a reaction, but this was a substantial increase over the average 1.3 percent of the previous five years. In the elections from 1970 on—all years in which large percentages of voters named inflation the nation's most serious problem—the greater support they were predicted to give to the Republicans was not forthcoming. These figures suggest that through the 1960s the Republicans may have held an edge in being perceived as better able to handle inflation. This, of course, was a period when inflation was so mild few people were much concerned about it. But in keeping with Okun's reasoning (1973), any favorable image voters may have had of the Republicans in this regard eroded in the face of the large acceleration in the inflation rate during the Nixon and Ford presidencies.

Similarly—at least in the elections analyzed in Tables 4 and 5—respondents who believed more government programs were needed to alleviate social ills did not appear to differ in their voting decisions from those who believed too much government spending or taxation was to blame for the nation's difficulties.[7] It should be noted, though, that the former category was very small from 1972 on, and that the latter categories never contained more than a small percentage of respondents. The final categories were composed of those respondents who mentioned some general or miscellaneous economic problem. For the most part, such concerns had no effect upon voting. The only glimmer of an impact occurred in 1972, when voters in this category gave greater support to the Democratic presidential nominee.

One last point deserves attention, and that is that the policy-oriented hypotheses receive more support in the earlier years of the congressional elections series than in the later years. In the first three or four equations the hypotheses do not fare badly. As mentioned earlier, the Republicans paid dearly for the 1958 recession, as did the Democrats for inflation in 1966. And although not statistically significant, the signs of the national inflation and unemployment terms were in the predicted direction as late as 1970. To be sure, this first set of elections contain some anomalous results as well, e.g., the effect of the commitment to more government programs in 1960 and in the 1966 concern over national unemployment. On the whole, though, results of the first half of congressional elections equations compose a somewhat attenuated and more erratic version of the pattern of findings obtained in the analysis of presidential elections.

This was not the case in the second half of the congressional series. In the five election equations beginning with 1970, only one national problem—more government spending in 1976—is statistically significant, and its sign is pro-Republican in direction. As the virtually random patterns of the signs of the various terms indicate, after 1970 the particular economic problems cited by respondents did not appear to affect their congressional voting decisions in any consistent fashion. Admittedly, the break between the first set of elections and the second set has probably been exaggerated. As one looks across the entire series in Tables 4 and 5, however, it seems clear that voters' perceptions of national economic problems have come to have virtually no impact upon their voting decision in congressional elections.

These findings are consistent with the evidence showing a "denationalization" of congressional elections over the past few decades (Burnham, 1974). Most researchers in this area attribute this denationalization to a substantial increase in the electoral advantage enjoyed by incumbents (Mayhew, 1974). Although this advantage is usually seen as producing comfortable winning margins, the greater importance of incumbency would also appear to make congressional elections less sensitive to cross-district national forces—or more precisely, to voters' perceptions of particular national economic problems.

Summary and Conclusions

The main theme running through the analyses undertaken by this study is that they repeatedly found important differences between the issues of inflation and unemployment. There were large differences between racial and occupational groups in respondents' propensity to name unemployment their most pressing personal problem. There were no such differences regarding inflation. These same group differences were also present, to a lesser degree, in the propensity to name unemployment the nation's most serious problem. Again, there were no such differences on inflation. And there are differences between inflation and unemployment in the very nature of the costs they impose on people. The survey data in this study were fully consistent with the findings of the economics literature: unemployment entails serious objective economic costs; but the major costs of inflation, at least in the short run, are the more intangible ones associated with living in a more uncertain and anxiety-provoking world.

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[7] Findings concerning the effects of these public sector issues were markedly different in the 1964 and 1968 elections, however. In these years there were large percentages of respondents in these categories, while the percentages in the inflation and unemployment categories were very small. Furthermore, estimates for the more government programs terms were strongly pro-Democratic in direction, while voters in the too much government spending and taxes categories in 1964 and 1968 voted strongly Republican.

Finally, inflation and unemployment had different effects upon voting. The inflation side of the policy-oriented hypothesis garnered only weak, scattered support. There were a few instances at both the personal and national level of inflation-sensitive voters supporting the Republicans, but that is all. In contrast, there was a mass of evidence showing voters reacting to their concern over unemployment by voting more Democratic. Voters who had personally experienced unemployment gave a modest but consistent boost to Democratic candidates in virtually every election. This effect was heavily supplemented in years of high unemployment by the large percentage of voters who felt unemployment was a serious national problem and consequently voted heavily Democratic as well. Thus in terms of the nature of its costs, its distribution, and its ramifications for party support and voting behavior, unemployment coincides neatly with the lines of cleavage drawn at the time of the New Deal. Inflation does not.

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COMMUNICATIONS

ON ARTICLES

Comment on Bensel
(Vol. 74, September 1980, pp. 734-44)

TO THE EDITOR:

The article by Richard F. Bensel, "Creating the Statutory State: The Implications of a Rule of Law Standard in American Politics," was interesting to me because I have always been fascinated by both Frederick Hayek and Theodore Lowi. But I question two points in the article.

The first point is a nit I would like to pick about federalism. Bensel makes a good case for the incompatibility of the rule of law with cooperative federalism as practiced in the U.S. today (p. 737). However, his assertion that the rule of law "favors a unitary national state over a mixed federal-state alternative" (p. 737) does not make an important distinction. Dual or layer-cake federalism in which certain reserved powers are delegated exclusively to state governments and other responsibilities are handled by the national government acting alone would be, in theory, completely in harmony with the rule of law. In this kind of federalism there would be no mixing of federal and state standards, a mixing which is incompatible with the rule of law.

Whenever there are more government levels involved, the possibility of ambiguity as to which has control over what activity is increased. Therefore, I would argue that unitary system is marginally more compatible with the rule of law than dual federalism, but marble-cake or picket-fence, cooperative federalism is clearly incompatible with this standard.

The second point concerns the idea of a responsible two-party system. The last paragraph of the article asserts that "supporters of the rule of law position seem to be at odds with those who advocate a strong, national two-party system" (p. 743). This contention does not follow from the empirical data presented in the article. These data show that party leaders, chairs of congressional committees, and presidents recently have not favored strengthening the rule of law standard as defined by Bensel. This suggests that the existing leadership in our weak, non-responsible party system does not support a return to this standard in America; but perhaps they would if held responsible to any standard—e.g., party responsibility—other than the whims of vocal constituent groups and the perquisites gained from close and enduring alliances with powerful forces in the career bureaucracy. There are no incentives in our current system to encourage adherence to the rule of law. Quite the reverse: party leaders, especially presidents, are encouraged to keep options open for future negotiation among interests. In a responsible party system, the major goal of party leaders is to carry out the party platform, thus assuring their own renomination by the party organization. Leaders will find the rule of law standard a handy, if not necessary, tool with which to accomplish this goal.

The article really does not address the question of conflict between rule of law and responsible parties. I believe that a direct comparison of the important features of a responsible party system and the rule of law will show that these two are compatible and even reinforce one another.

The rule of law statutory criteria listed by Bensel are:

1. The general rule contained in a statute must be prospective, not retrospective.
2. Statutory language must be clear, specific and publicly advertised. The statute must minimize administrative discretion.
3. The general rule contained in a statute must treat individuals equally in classification and enforcement. The objects of state policy should be classes and categories of citizens or behavior rather than specific individuals or isolated situations.
4. Statutes must be drafted by a government body (usually the legislature) not involved in direct enforcement or implementation.
5. All administrative decisions must be subject to judicial review for compatibility with statutory specifications (pp. 736-37).

The criteria for a responsible party system are as follows:

1. Parties must be committed to a range of government policies.
2. Party platforms must clearly state these policy preferences in advance of elections.
3. The party with the most votes must get complete control of the government.
4. The elected public officials of the party must carry out the party platform.

The requirement to write out laws to specify what government policy should be, rather than mushing one's way through with administrative compromises, would seem to be a necessary means of implementing point 4 of the responsible party criteria. Even point 5 of the rule of law statutory criteria helps responsible parties. If the point of judicial review is to assure that the bureaucracy is carrying out the law as written by the legislature, then rule of law point 5 has the effect of using the courts to make the bureaucrats carry out the party platform. Thus a rule of law standard brings judges and civil servants into line with the party. How else could this be done without compromising merit selection of public employees and an independent judiciary?

It also seems clear that a responsible party model *is* incompatible with the interest group liberalism decried by Lowi. How can a party define its policies clearly in a platform and then carry them out in the context of the legislative and bureaucratic negotiation among interest groups which characterizes our system in the absence of the rule of law? Interest group liberalism flourishes within a loose, weak, non-responsible, faction-ridden party system. It cannot survive alongside strong parties. Or perhaps it should be stated the other way around: parties cannot be strengthened or at least made responsible without the rule of law standard.

THEODORE ARRINGTON

University of North Carolina, Charlotte

Reply

TO THE EDITOR:

Theodore Arrington's communication raises two questions: the relationship of the rule of law standard to federalism and the implications of the empirical findings I reported for responsible party government. Concerning federalism, we are in agreement that the rule of law standard prefers a unitary, nationalized policy to a cooperative federal-state alternative, due to the discretion allowed local administrators under the latter arrangement. Arrington questions the relationship of the standard to "dual sovereignty": a federal system in which the state and national governments exercise absolute control over mutually exclusive legislative domains. I don't think we disagree on this point, but I would resolve the question in a slightly different way.

In the article, my goal was to develop an empirical standard which would measure the degree to which competing legislative alternatives conformed to a rule of law. For this reason, I considered only those policy alternatives which commonly arise as choices on the floor of the House of Representatives. Congress can choose between nationalized and mixed, federal-state alternatives by substituting one administrative structure for another. In the normal consideration of legislation, a "dual-sovereignty" alternative cannot arise because it is a systemic principle embodied in a constitution and not an attribute of an individual policy; only a constitutional doctrine enforced and arbitrated by a supreme judicial body can create and sustain a "dual-sovereignty" system. Congress cannot create mutually exclusive domains of legislative sovereignty through the passage of individual policies alone and, even though Congress can refuse to legislate in certain areas, inaction provides no guarantee to the states that Congress will not intrude upon state powers in the future; congressional inaction does not automatically imply that the states will enact a local alternative to the foregone national policy; nor can Congress confer powers upon the states that the Constitution doesn't give them (e.g., over interstate commerce). For these reasons, "dual-sovereignty" arrangements do not normally arise in the national legislative process and I did not consider them in the theoretical discussion of the scope of the standard.

There is only one legislative situation in which a choice between a "dual-sovereignty" alternative and a "nationalized" proposal could be presented to Congress. That one situation would arise if Congress were to consider a constitutional amendment to reestablish dual sovereignty as a constitutional principle. The rule of law position in that situation would favor the status quo and oppose the dual-sovereignty amendment because a dual-sovereignty principle would create an inherently ambiguous barrier between national and state authority. As the first century and a half of American judicial history demonstrates, such a barrier greatly expands the discretionary power of the federal judiciary and increases the instability of constitutional interpretation. In contrast, the prevailing interpretation of the Constitution clearly provides for the supremacy of national law. Thus we agree that the rule of law standard would prefer a unitary state to dual sovereignty, though Congress will probably never encounter the choice.

With reference to the second point concerning responsible party government, both the leadership of the committee system and the members of the president's party have been independently hostile to the rule of law standard. We both agree that the behavior of the modern committee system is consistent with "pluralist" ideology. We may disagree on the necessary connection between responsible parties and support for the standard. In the article, I presented evidence that relative support for the standard by the two parties depended on control of the White House (Table 1, p. 740) and that support for the standard, while related to support for the president's positions, varied between members of the two parties even when the propensity to support the president was taken into account (Figure 1, p. 741). In the absence of a modern example of a responsible party, I believe this strong relationship between control of the executive branch and opposition to statutory articulation provides supporting evidence for my conclusion. I agree that there is no apparent *logical* inconsistency between the standard and responsible parties as they have been ideally described. My response is that whenever the party system has produced a clear electoral choice, a decisive outcome, and a subsequent implementation of the victorious party's program, the result has been an orgy of delegated power (e.g., the New Deal, the 89th Congress during the Johnson years after 1964, etc.). In practice, "complete control of the government" by something approaching the responsible party ideal has meant the subordination of Congress (and the law) to the president (and executive discretion). Even in the absence of a formal connection, this pattern seems strong enough to warrant the implication of incompatibility. This conclusion is not affected by the apparent hostility of responsible party government to a strong committee system.

However, because we have no information on the attitude of Congress toward statutory specification during other historical periods, my willingness to draw the implication of incompatibility between responsible parties and statutory articulation is only tentative. A similar analysis of congressional behavior during an era in which congressional parties were relatively strong, the committee system was weak, party professionals controlled nominations, and the Supreme Court enforced a rule of law standard as a part of a separation of powers doctrine would produce more conclusive evidence concerning the relationship of the party system to the quality of legislative law. The progressive era from 1895 to 1915 would meet these requirements and, in addition, results drawn from that period would allow us to trace the origins of the discretionary state.

RICHARD F. BENSEL

Texas A&M University

Comment on Morgan and Pelissero (Vol. 74, December 1980, pp. 999-1006)

TO THE EDITOR:

In our article, "Urban Policy: Does Political Structure Matter?" John Pelissero and I examined the oft-researched question of the effect of urban government structure on public policy. From an analysis of 11 small and medium-sized cities that had changed their government structure between 1945 and 1978, we concluded that municipal reforms have virtually no impact on city taxing and spending policies. This effort was the first to analyze the effects of local government structure on policy using a quasi-experimental time-series design. We had no illusions then nor do we now that our research would be the last word on the matter. In fact, much to our surprise and embarrassment, a mistake in our article has been discovered. However, we are quite thankful that this error does not affect the basic conclusions of the paper.

Roland E. Dufault, senior staff assistant to the city manager of Worcester, Massachusetts, correctly notes that his city had been misclassified in our Table 1. We showed Worcester as having changed from a completely reformed city (council-manager, at-large elections, nonpartisan ballot) to a totally unreformed city. Worcester, in fact, did modify its form of government in 1950, but in just the *opposite* way from that indicated: from unreformed to completely reformed. Because of an unfortunate transposition in coding from the draft form to the final table, the government structure information shown for Worcester was backwards, and was wrongly included among the group of cities changing from reformed to unreformed structures.

This error necessitates a complete reanalysis of the data to determine how such a mistake affects our findings. First, a change in cities is required. Worcester remains in the data set, but now as a city that changed its structure from unreformed to reformed. This means, of course, that a new control city must be found to take the place of Hartford, Connecticut, which had been used initially to match Worcester. We now need an unreformed city (Hartford was reformed). After carefully considering the criteria for choosing the control cities, we have selected Providence, Rhode Island as the best match for Worcester. The appropriate data for Providence are as follows (to fit the original Table 1, p. 1002):

	Economic Base	Government Structure
9. Providence	Mm [manufacturing]	MC-W-P

Providence is completely congruent with Worcester on school and welfare assignment.

With Providence now included as the unreformed match for Worcester, we can undertake our reanalysis. To repeat briefly, our approach requires grouping the cities into four categories—those becoming more reformed (N = 9), those changing to less reformed (N = 2), and two groups of control cities. The interrupted time-series analysis includes seven revenue and expenditure variables for an eleven-year period—the year of change and five years before and after the intervention. The various dependent variables are averaged for each group of cities. The prediction equation employs a linear measure of time, a dummy variable to represent the alteration in level (intercept) following the reform, and a measure to represent the post-intervention change in slope. If the intercept and slope changes are not negatively significant (at .01), we accept a null hypothesis that reformed cities do not tax or spend less than their counterparts.

How much do the results vary upon reanalysis? Very little. A total of 28 equations are calculated, yielding 56 slopes and intercepts to be checked. The original analysis had produced nine equations with significant slopes and intercepts, only four of which were for cities undergoing structural change. (No autocorrelation for experimental cities was detected then or now.) Of the four originally significant changes, all were positive, indicating more than expected increases in taxing or spending. But two of these equations were for cities that became less reformed, contrary to the null hypothesis. In the reanalysis the following slopes or intercepts are significant (at .01 level):

more reform cities	2
less reform cities	1
control cities	4

Now, only three experimental groups show a significant change. For the two groups of more reformed cities, both regression coefficients are positive, indicating more than expected spending increases. The one significant change for an unreformed group also reflects a positive slope, suggesting, of course, that reforms do matter. The control group for this change (in an equation analyzing general expenditures) does not reflect a statistically significant intervention slope, although the sign is positive. In effect, then, only 1 of 28 interventions (4 percent) fails to support the null hypothesis. This figure compares with 7 percent in the original analysis.

A direct comparison might also be made of slope and intercept coefficients that changed from the original analysis. This reveals that only one originally significant coefficient (slope or intercept) for experimental cities became insignificant (one control group also became insignificant). No other changes occur. Again this result overwhelmingly confirms the original findings.

Although we regret the error in our original analysis, the reanalysis it occasioned confirms even more strongly the results initially reported. Again we conclude that a change in city government structure has very little effect on municipal taxing and spending behavior. The modified Table 2, presenting the new analysis, will be furnished upon request.

DAVID R. MORGAN

University of Oklahoma

Comment on White
(Vol. 74, December, 1980, pp. 1007-17)

TO THE EDITOR:

In "Reason and Authority in Habermas," Stephen White covers more important material more lucidly in less space than I would have thought possible. In many respects it is a fine essay. Yet I object to his conclusion, which I take to be that Habermas' communicative ethics is neither intentionally nor inherently authoritarian because it has little to say about how institutions should be organized. I object because White's conclusion is only half the story. Precisely those aspects of philosophic discourse (White calls them "fallibilism and tolerance") which should find institutional expression are not readily translated by Habermas into institutional form either (White, p. 1013). Habermas' institutional irrelevance is a mixed blessing.

Habermas has actually been somewhat more concerned with applying discourse to real-world situations than White suggests (on "steering discourses" see 1970, pp. 72-80; on "advocacy discourse" see 1975, pp. 112-17). I will focus upon the status of discourse in situations of political conflict, which Habermas examines under the rubric "the objectivating application of reflexive theories" (1973, pp. 37-40). (This application of discourse is part of Habermas' response to Karl-Otto Apel's claim that the emancipatory cognitive interest tends to politicize science [*Wissenschaft*] by giving its interest in emancipation a particular political content [Apel, 1974, pp. 338-42; Habermas, 1973, p. 24, n. 42]. Habermas meets this objection, but raises the problem discussed here.)

Habermas recognizes that political conflict cannot be organized on a discursive basis, for the opponent generally cannot be drawn into meaningful dialogue. Consequently, it is often the case that those who would change society must engage in strategic action. However, it is important, says Habermas, that those who would change society organize themselves discursively. They should decide among themselves under discursive conditions what the goals of their action should be, and what means should be employed. The discursive organization of social activism realizes, on a small scale, the end of the revolution, a free and open society, in its means (1973, pp. 37-40).

The discursive organization of social activism has a peculiar consequence. Those who so organize themselves, suggests Habermas, cannot help but see themselves as being in the sole possession of enlightenment, even though this view is false, as its basis in discourse is incomplete, for the opponents have not yet been discursively consulted (1973, p. 40). This is a puzzling claim. I have little difficulty, for instance, imagining myself thinking along the following lines while about to engage in social action: "I believe I am right, though I am not sure; it could be that my opponents are correct. However, it is necessary to act; I have analyzed the situation as best I can, so here goes." The question is why this line of thought, which might be called a thoroughgoing fallibilism of action, seems unavailable to Habermas.

It must not be forgotten that for almost fifteen years Habermas has sought to develop an epistemological social theory (1971, pp. vii, viii). Discourse has played a number of roles in this project, but in either the foreground or background the claim has always been present that a discursively achieved consensus is a justified one. This seems to mean that consensus under the ideal speech situation is indistinguishable in its force from what used to be called philosophical first principles, or sufficient grounds. Strategic political action, however, is cut off from discursive justification. This leaves it vulnerable to the claim that it is merely a form of "decisionism" (*Dezisionismus*). Yet it is precisely the arbitrary character of "decisionism" that Habermas has directed so much of his dazzling brilliance towards overcoming. In this circumstance what he seems to do is to suggest that if social activists organize themselves discursively they can and perhaps must act as though the rightness of their actions is warranted by the "promissory note" that one day all will be discursively consulted. The idea of a "promissory note" which may be redeemed after the revolution explains *why*

> the strategic action of those who have decided to engage in struggle, and this means to take risks, can be *interpreted hypothetically* as a retrospection which is possible only in anticipation. . . (1973, p. 40, emphasis added).

There is no alternative in Habermas' work, no space, as it were, between grounded and arbitrary claims. It is for this reason that he sometimes writes as if the "halo" of discourse could be transferred to nondiscursive situations. In fact, the following scenario—precisely the type White wishes to exclude—is readily imaginable. Social activists through clever strategic action have turned the tide and begun to win the revolution. At this point former opponents begin in droves to "recognize themselves" in its goals, and rush to participate, first in "therapeutic discourses" (re-education), and then in genuine discourse designed to legitimate the new order. And who is to say they are acting in bad faith? Seemingly genuine conversions of this type occur frequently. However, the truth and rightness of the winner's claims is hardly vindicated by the agreement of those who—if they failed to agree—would be excluded from those discourses which would run the new order.

This admittedly heavy-handed scenario does not support the claim that discourse is inherently authoritarian. For instance, the "negative freedoms" of those who continued to dissent might be stringently protected (though it is not so clear that White's interpretation of participation as a "preferred freedom" would be terribly relevant under these circumstances) (p. 1016). But, incipient authoritarianism is perhaps not the real issue in any case. Rather, it is that the fallibilism and tolerance of philosophic discourse should be institutionally reflected in procedures which allow for error. Habermas' failure to devote much attention to this problem reflects, I suggest, the momentum (though not the intention) of a style of thought in which all decisions are ultimately either grounded or equally arbitrary. Seen in this light, White's defense of Habermas on the basis of his limited institutional relevance is ironic.

In those societies in which peaceful change is possible, perhaps Karl Popper's (today not very fashionable) suggestions regarding "piecemeal social engineering" as the institutional embodiment of fallibilism need repeating. In those societies in which peaceful change is not possible, we must at least remind social activists that they are not operating with a "promissory note" of any kind. We should do so if for no other reason than that not all those who suffer, and certainly none of those who die along the way, will be available for a post-revolutionary consensus.

C. FRED ALFORD

University of Maryland

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Reply

To the Editor:

I would like to thank Fred Alford for his thoughtful response to my article. I must, however, disagree with the thrust of his criticism. First of all, Alford asserts that I have argued that Habermas' thought is nonauthoritarian "*because* it has little to say about how institutions should be organized" (emphasis added). Habermas has indeed offered relatively few specific recommendations about institutions, but that fact is not the *reason* for the nonauthoritarian character of his thought. The reason is, as I argued in my article, that his communicative ethics necessarily implies a strong emephasis on fallibilism and tolerance. If this argument is correct, then it would seem that communicative ethics could support institutional arrangements in some ways not so different from the Popperian-based ones Alford recommends.

Alford, however, does not want to accept such a conclusion. He claims that Habermas' emphasis on fallibilism and tolerance cannot be translated into institutional arrangements. The difficulty in this regard reflects, according to Alford, the fact that in Habermas' thought "all decisions are ultimately either grounded or equally arbitrary." Presumably this means that Habermas cannot endorse a "thoroughgoing fallibilism" because he feels that some decisions in politics are ultimately fully justifiable on discursive grounds. To support this line of argument, Alford attempts to show how Habermas' remarks in *Theory and Practice* (1973, pp. 37-40) can easily be used to support a heavy-handed sort of politics, in which revolutionaries can justify whatever action they take as long as they issue a "promissory note" to submit themselves to the ultimate source of justification, a post-revolutionary discursive consensus. Barred from participation in such a consensus will be those opponents who have still not seen the revolutionary light and, of course, those opponents who have been killed along the way.

This scenario in fact misrepresents Habermas' remarks on political action. He suggests only that political actors should *interpret* the political struggles in which they find themselves in light of the goal of possible social arrangements in which greater discursive formation of political will is allowed. These interpretations, however, can only "open up a perspective" for strategic political action; they *"cannot claim a justificatory function"* (1973, p. 40, emphasis added). In other words, political actors cannot justify violence against others simply by issuing a "promissory note."

Stephen K. White
Virginia Polytechnic Institute
and State University

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On Book Reviews

Comment on Sartori's Review
(Vol. 74, December 1980, pp. 1060-61)

To the Editor:

Readers of Giovanni Sartori's review of my *Democratic Political Theory* (especially those who have not read the book) may be interested to know how I would respond to his claim that my method is "both maddening and as feeble as can be."

Sartori's major criticism is that my classification of democratic theories lacks a criterion. From this flaw others follow. He is mistaken. He has overlooked a critically important and oft-repeated statement and related distinction. As is clearly stated, the classification is based upon two criteria, both of which relate to human motivation. One (see for instance the general heading for rows in chart 2, p. 169) is *prescriptive,* dealing with the motivation the theorist in question holds ought to prevail in politics. The other (the general heading for the columns in the same chart) is *descriptive,* and refers to the political motivations (alternatively called "key values") the theorist believes actually do prevail. This classification is presented as provisional and crude, but useful. It is stated that some theorists deal only with one or the other of these elements. It is also pointed out that of the sixteen categories provided by the four-by-four matrix most theorists tend to fall in one of four—the one that assumes as a dominant political motive that motive the theorist in question values most highly.

These four categories approximate "pure types," so it is not surprising that few theorists correspond exactly to any one of them; but I see no ground for suspecting that "authors can be fitted in almost anywhere." The four categories, together with suggested candidates for each, are as follows: "radical individualislm," James Mill; "public interest theory," John Dewey; "rights and duties theory." John Plamenatz; and "collectivist theory," Karl Marx. I believe it is clear that no one of these authors could be fitted into any of my categories other than the one in which I have placed him. Nor am I uncomfortable, with these large major categories, about finding Locke and Rawls in the same box. Surely they have more in common with each other than either does with James Mill, John Dewey, or Karl Marx. That the categories sometimes merge into each other is not strange: it is as it should be. Some individualists are more individualistic than others; some public interest theorists grant more to individualism than do others (both prescriptively and descriptively). I see nothing "artificial" about admitting these facts; rather, a set of categories that did *not* admit of such qualifications would be artificial, departing from the nature of human beings and failing to register the complexity of matters political. Nor is it odd to have separate categories for Dewey and Marx, while admitting the fact of certain "natural affinities" between them. Indeed, the existence of meeting points between the types of theory is testimony to their logical interconnectedness, an element that Sartori finds lacking.

As to my consideration of Rawls, I treat him as a "deontological" theorist, by which I mean one whose main stress is on "rights and duties." I do not mean that he deals *only* with what he considers *correct* rights and duties and not at all with what the average individual's behavior suggests he *believes* to be his rights and duties. That should be clear from my statement, quoted by Sartori, that Rawls' theory is "stronger on the normative than on the operational side" (p. 195), as well as from my statement that Rawls' theory is "*largely* prescriptive, while Plamenatz's is *mainly* descriptive" (p. 193, emphasis added). When Rawls moves to discussing the application of his normative theory to actual institutions, one may "properly" wonder whether the operation of those institutions that he recommends would "make a good fit" with his prescriptive principles. At the very least this would alert one to the fact that what he is saying applies only to a society in which the citizens fully subscribe to his theory of justice and behave accordingly. My discussion of Plamenatz on the immediately preceding pages should prepare the reader, if preparation is necessary, for the fact that a deontologist may make use of "operational" theories of rights and duties as well as more narrowly normative ones.

Sartori's second and less central charge is that my distinction between "justificatory" and "operational" theories is "conceptually diffuse" and no improvement over the more familiar "normative"/"empirical" dichotomy. My concern was to find a positive term to cover all non-justificatory theories, not all of which are empirical. Necessarily, it would cover a lot of ground. Appropriate sub-categories are available for whatever purpose may be at hand. What one would not gather from the review is that more specific concepts *are* used when called for—particularly when it comes to confirming or disconfirming "grand generalizations." How I do deal with the adequacy of various types of theory as judged in the light of empirical evidence may be found in any of the relevant sections of chapters 6-12. See, for instance, pages 268-77.

No sensitive author can have a work seriously misread by a scholar of Sartori's standing without wondering whether he or she has contributed to that lack of understanding. If the points I have made above do not stand out clearly enough in the book, I welcome this opportunity to underline them.

J. ROLAND PENNOCK

Swarthmore College

Reply

TO THE EDITOR:

Roland Pennock's important *Democratic Political Theory* is—as he will, I trust, agree—an *ingens sylva,* a wilderness of richness. As such, it affords many readings and also, alas, possible misreadings. But I did not misread the point that Pennock's "classification is based upon two criteria." Perhaps I take the notion of classification more seriously than Pennock; but, as a matter of logic, it behooves me to insist: a classification cannot be based on two criteria. Conversely put, two criteria cannot yield classes that are mutually exclusive, and therefore cannot yield a classification. To be sure, we often combine (in typologies, taxonomies or pure and simple mappings) two or more criteria. But in such cases the logical power of classification is lost, and the entailed risk is that obfuscation may prevail, in the end, over clarification. In any event, I do welcome Pennock's response to my review. If I have been somewhat unfair to the author, it is only fair that the author should respond. It also pleases me to take this opportunity to say that I have always held Pennock in very great esteem, and that disagreements on a particular work do not affect in the least my admiration for his scholarly accomplishments.

GIOVANNI SARTORI

Columbia University

Comment on Tufte's Review
(Vol. 74, June 1980, pp. 567-68)

TO THE EDITOR:

Unfortunately, Edward Tufte's review of our book *Can Government Go Bankrupt?* is vitiated by his disregarding the contents of the book. Two examples will suffice.

Tufte states that we assume citizens lack the sense to appreciate the value of welfare state benefits. In fact, we argue the opposite, devoting the whole of chapter 3, entitled "Everybody Benefits," to explaining the popularity of welfare state programs.

Tufte states that our "remedy is reduced government spending" (p. 567). Assuming Tufte is familiar with the difference between a decrease in public expenditure and a reduced rate of increase, we conclude that he must not have read the 12-page section in which we recommend the latter, captioned "How to Put the Brakes On."

Fortunately, persons such as Alan Peacock, Aaron Wildavsky, and even a Labour Cabinet minister, have read the book and commented favorably about it. We leave third parties to judge whether their opinion or that of your reviewer is to be taken more seriously.

RICHARD ROSE

University of Strathclyde

B. GUY PETERS

Tulane University

Reply

TO THE EDITOR:

Rather than defending their book by substantive argument, the authors say naughty things about the reviewer and then round up a hapless trio of book-chat authority. Although A. Peacock, A. Wildavsky, and a Labour minister have yet to endorse my review, I will stand by every word. The cited chapters do not refute in the slightest the two sentences of the review now grumbled about. My evaluation of the book remains (p. 568):

> Skepticism about the growth of the public sector has been advanced with far greater depth, sophistication, and clarity by many including George Stigler, William Niskanen, Assar Lindbeck, the public choice crowd, and indeed the median *Public Interest* essayist. . . .
>
> Aaron Wildavsky is quoted on the jacket flap: "This splendid book is essential for understanding the crisis of governability in the welfare state." It is nothing of the kind; rather it is the sort of book that leads economists to look upon political science as political scientists look upon sociology.

EDWARD R. TUFTE

Yale University

Comment on Cantori's Review (Vol. 74, March 1980, p. 224)

TO THE EDITOR:

The review of my *Egypt's Uncertain Revolution Under Nasser and Sadat* by Louis J. Cantori opens with the observation: "The merit of this book lies in the lucidness of its writing and the relative comprehensiveness of its coverage of Egyptian political history since 1952." Alas, the rest of the review is an argument that the book has little to offer the specialist in comparative politics or Egyptian affairs.

Cantori is, of course, entitled to his view. In fact, I think it is a revealing one about prevailing notions of the specialist. But I do feel obligated to point out that the evidence presented as the basis for the judgment consists of an incorrect assumption and a misreading.

Cantori writes that it is my "ignorance"—"unintentionally revealed" as a comparativist, "profound" as an orientalist—that excludes me from the circle of specialists. Consider now the reasoning that produced that judgment.

In the first instance, I am taken to task for writing in my preface that it "makes no effort to ensnare Egypt in yet another formal and ahistorical taxonomy" (p. vii). It is in this comment that Cantori finds "an unintentionally revealing confession of ignorance of dynamic theories of comparative politics (e.g., S. Huntington, *Political Order in Changing Societies*)." Cantori is deluding himself. If such theories do exist, they must be among the most closely guarded secrets in American social science. Cantori does not cite his own work. But I must protest that the reference to Huntington is unfair. After all, in his well-known "The Change to Change" article, Huntington did label clearly his own "transitional approach" the "*very modest first steps toward* the formulation of general theories of political dynamics" (1971, emphasis added). Readers of *Political Order* with an interest in Egypt welcomed the modesty. In that book Huntington developed the typological notion of Egypt as "praetorian society." It was precisely this interpretation of modern Egypt that stimulated my determination that *Egypt's Uncertain Revolution* would not be another arid, artificial, and misleading exercise in classification. (As a biographical note, I should add that my comparative work on Egypt began many years ago with a seminar paper for Huntington who subsequently chaired my general exams, and then got me my first job—in comparative politics!)

But it is the case of my inadequacies as an orientalist that reveals the reviewer's power as a critic. Cantori writes that my point of view "shows a profound ignorance of Egyptian poetry, religion, and music from the 1920s onwards." He bases his extraordinary view on a misreading of one paragraph. In a brief historical introduction I offer a logical reconstruction of the colonial situation in Egypt that led to the rise of nationalist resistance. My argument is that the choices offered to Egyptians in such a situation were intolerable and therefore rejected. In particular, the racist character of the British view of the Egyptians is underscored:

> In his classic study of the colonial experience Albert Memmi observed: "It is remarkable that racism is a part of all colonialisms under all conditions." British colonialism in Egypt proved no exception, despite the undoubted achievements registered by the British in the spheres of agriculture, finance, and communications. Lord Cromer, the all-powerful British proconsul from 1882 to 1907, best recorded the racist underside of British colonial rule. In his memoir, *Modern Egypt,* Cromer eloquently defended Britain's role in civilizing "barbarous" Egypt. To read Cromer is to understand the cultural amnesia of the colonized Egyptian: better to forget the "coarsenes" of his literature, the "cruelty" of his mores, and "the lack of harmony" of his music. To become human is to leave this cultural wreckage and barbarism of a "subject race." But to go where? The Egyptian cannot retrieve his humanity by assimilation, for to adopt the ways of the colonizer is to become the clever monkey, the good imitator, but never the genuine article. Cromer unintentionally has revealed the essential frustration of the colonial situation: for the colonized Egyptian there is no way out.

Here is Cantori's reading of this paragraph:

> Consider, for example, Baker's use of Cromer (the arch-colonialist) as a person who defined Egyptian culture in terms of its "coarseness," "cruelty," and the "lack of harmony" of its music. Egyptians are then said to have undergone "cultural amnesia" at the presumed inspiration of Cromer's remarks. This point of view shows a profound ignorance of Egyptian poetry, religion and music from the 1920s onwards.

I was pleased to note that Cantori's listing of the charmed circle of "truly original scholars of modern Egypt" includes no Egyptians (and no Soviets). In my own unhappy exile, it seems, at least, I have not been deprived of all my friends.

RAYMOND BAKER

Williams College

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Reply

TO THE EDITOR:

Two brief comments regarding Raymond Baker's response to my review of his *Egypt's Uncertain Revolution Under Nasser and Sadat.*

First, the point regarding my criticism of the book from a comparativist point is not the "theological" one of whether "dynamic theories of comparative politics" exist or not. It is rather Baker's failure to direct his concerns in the book to any social science generalizations whatsoever (short of a "genuflection" on pp. 11-13). Second, Baker has usefully made my point for me by quoting himself: "To read Cromer is to understand the cultural amnesia of the colonized Egyptian. . . ." I think that a reader is entitled to interpret this as meaning that Baker meant to castigate Cromer à la Memmi *and* to state that Cromer was essentially correct, Egyptians were undergoing "cultural amnesia."

My only comment would be that the major theoretical and literary works of the twenties and thirties, e.g., Shaykh Ali Abd al-Raziq, *Al-Islam wa usul al-hukm* (1925), Tawfig al-Hakim, *Awdat al-Ruh* (1926-27), Taha Husayn, *Fi al-shir al-jahili* (1926) and *Mustaqbal al-thaqafa fi misr* (1938) may have reflected intellectual controversy of a high order and even a crisis of cultural direction (Arab Islamic, Egyptian, Greco-Roman or Mediterranean) but in no manner can they be said to constitute "cultural amnesia."

LOUIS J. CANTORI

University of Maryland, Baltimore County

Clarification of Haviland Review
(Vol. 74, December 1980, pp. 1100-01)

In response to two letters about his review of *Decision on Palestine: How the U.S. Came to Recognize Israel* by Evan M. Wilson (Hoover Institution, 1979), H. Field Haviland, the reviewer, submitted the following clarifications:

1. The citation of 1914 as the date of the Balfour Declaration was a typographical error. It should have been 1917.

2. The United Nations General Assembly's resolution of November 29, 1947, greatly advanced the target date, by which time the Arab

and Israeli states were to become independent, from September 1, 1949, recommended by the U.N. Special Committee on Palestine, to October 1, 1948.

3. It was in 1973 that President Truman made his comment about comparing the pressure exerted on him regarding the Palestinian question and the dismissal of General MacArthur.

EDITORIAL NOTE

The following articles have tentatively been scheduled to appear in the September, 1981, issue:

John Higley, Harvard University, and Gwen Moore, State University of New York at Albany, "Elite Integration in the United States and Australia"

Calvin C. Jillson, Louisiana State University, "Constitution-Making: Alignment and Realignment in the Federal Convention of 1787"

Robert J. Morgan, University of Virginia, "Madison's Analysis of the Sources of Political Authority"

Timothy J. O'Neill, Tulane University, "The Language of Equality in a Constitutional Order"

Frederick G. Whelan, University of Pittsburgh, "Citizenship and the Right to Leave"

Alfonso J. Damico, University of Florida, "Dewey and Marx: On Partisanship and the Reconstruction of Society"

Jack L. Noragon, Ohio Legislative Service Commission, "Political Finance and Political Reform: The Experience with State Income Tax Checkoffs"

Bryan D. Jones, Wayne State University, "Party and Bureaucracy: The Influence of Intermediary Groups on Urban Public Service Delivery"

Virginia Sapiro, University of Wisconsin, Research Frontier Essay: "When Are Interests Interesting? The Problem of Political Representation of Women"

Irene Diamond, Purdue University, and Nancy Hartsock, Johns Hopkins University, "A Comment on Virginia Sapiro's 'When Are Interests Interesting? The Problem of Political Representation of Women' "

Erratum

We apologize for an error that appeared in Richard D. Shingles' article, "Black Consciousness and Political Participation: The Missing Link" (March, 1981). On p. 81, cell *b* of Model I, Table 2, should read: "Optimal high-initiative policy behavior."

The corrected table is presented below:

Table 2. The Joint Effects of the Sense of Internal Political Efficacy and Political Trust on Different Modes of Political Participation, Controlling for Prior Motivations

Model I: Given no other motivation to participate

		Trust in Government	
		High	Low
Internal Political Efficacy	High	*a* Optimal high-initiative allegiant behavior	*b* Optimal high-initiative policy behavior
	Low	*c* Low-initiative allegiant behavior	*d* Total withdrawal from conventional politics

Model II: Given prior motivation to participate

		Trust in Government	
		High	Low
Internal Political Efficacy	High	*a* Optimal high-initiative allegiant and policy behavior	*b* High-initiative allegiant and policy behavior
	Low	*c* Low-initiative allegiant and policy behavior	*d* Total withdrawal from conventional politics

Source: Compiled by the author.

BOOK REVIEWS

Political Theory and Methodology

Social Justice in the Liberal State. By Bruce A. Ackerman. (New Haven, Conn.: Yale University Press, 1980. Pp. xii + 392. $17.50.)

Bruce Ackerman's book will doubtless remind most readers of the works of Rawls and Nozick, and it can be expected to have a similar if somewhat lesser impact. Lesser, because Ackerman lacks the panache and wit of Nozick, the philosophical maturity and meatiness of Rawls. Similar, because Ackerman attempts the same sort of comprehensive theory of justice, broadly within the liberal tradition, as they did.

All three share a methodological commitment to "constrained performance," that is, to the attempt to generate principles of justice from a performance or action undertaken in specially defined or constrained circumstances. This method is to supply the positive normative principles which, it is felt, can no longer be supplied through an appeal to nature or history, the traditional appeals of previous political philosophies. For Ackerman, the performance is a dialogue, brought into existence each time the legitimacy of a power relation is challenged. Power relations, including rules for distributions of the goods of the world, can be justified if and only if they can be justified through a dialogue, carried on within the constraint set by the principle of neutrality: "No reason is a good reason if it requires the power holder to assert: (a) that his conception of the good is better than that asserted by any of his fellow citizens or (b) that, regardless of his conception of the good, he is intrinsically superior to one or more of his fellow-citizens" (p. 11). At first glance, it appears that Ackerman overstates the role of the dialogue performance in his own theory; for all the *talk* of dialogue, what he really means is that all power relations must be justified by arguments that conform to certain criteria (e.g., neutrality), a position weighing the act of dialogue no more than any political philosophy which attempts to justify power relations. The appearance to the contrary notwithstanding, the performance of the dialogue remains central to Ackerman's enterprise. Without the performance, it would be simply impossible to generate Ackerman's liberal principles of justice, or perhaps any principles of justice, for the neutrality constraint supplies only a basis for disqualifying certain kinds of claims, and not for qualifying any. In emphasizing the performance of the dialogue, Ackerman attempts to side-step this problem by beginning with claimants and their claims to power. Their claims can stand, Ackerman seems to say if they, under challenge from potentially competing claimants, can give reasons which do not violate neutrality. Thus Ackerman attempts to turn the corner, deriving positive claims from his negative neutrality constraint. The question nonetheless persists, whether or not Ackerman's dialogic turn succeeds.

With great boldness and some ingenuity, Ackerman applies his dialogic method to a wide range of difficult issues, including not only such standard ones as justice in holdings, but also areas like genetic engineering and liberal education. He proceeds in stages, first developing an "ideal theory," characterized by a number of unrealistic simplifying assumptions, and then a second-best theory, relaxing the simplifying assumptions. Ideal theory assumes a "liberal commander," entirely dedicated to enforcing justice, and able to do so through possession of a perfect technology of justice. While Ackerman professes great hostility to state-of-nature theory, he begins in a very similar way. He assumes a group of human beings deliberating over how to distribute holdings on a virgin planet, where all holdings can be expressed in terms of a perfectly fungible item called manna. Under such conditions, the only principle of initial distribution that can be justified is equality. Since no generation can claim priority over any other, all members of later generations have a right to holdings at least equal to those of members of the first generation. From this follow the principles of population policy and inheritance law. Ackerman's principles for governing economic relations are somewhere between Rawls' and Nozick's. He presses the issue of initial holdings much harder than Nozick does, and demands that each generation has a right to what the first generation had, but more like Nozick than Rawls, he does not press for the imposition of a just pattern in holdings at every instant. Free exchange is one of the liberal freedoms that, with qualifications, Ackerman defends. Ackerman's relatively moderate position has much that is attractive about it, but also some problems as well. How, in the real world, do we discover the baseline first generation and its baseline holdings? Relevant also is Nozick's complaint against theorists who assume the problem of justice is essentially one of distribution of preexisting goods, like manna.

Second-best theory attempts to adjust to the absence in the real world of either a perfect will or a perfect ability to achieve justice. In place of the commander, Ackerman develops the institutional mechanisms of liberal democracy. In a world where all liberal rights cannot be secured, Ackerman proposes the principle of equal sacrifice: providing that no citizen may be asked to sacrifice more of his or her ideal rights in the real world than other citizens. The liberal state is obliged to take various forms of affirmative action to prevent and remedy such inequities.

Ackerman attempts to justify his method of liberal dialogue via a lengthy critique of the contractarian and utilitarian traditions, which finds them both necessarily violating neutrality in a similar way. Ackerman's critique of his predecessors suffers, however, from his failure in the case of, e.g., Rawls to understand correctly the doctrine of the original position or even the second principle of justice (consider, for example, pp. 266-69).

The core difficulty in Ackerman's book, however, lies in the tension between the commitment to dialogue and the Neutrality constraint. While he looks to Socrates as the paradigm of the liberal philosopher, he excludes at the outset the quest that animated Socratic dialogue—the quest for knowledge of the good. Neutrality rules out such knowledge in advance. He attempts to justify that exclusion via a route which this reviewer frankly finds unintelligible. In his last chapter he argues that a wide variety of routes, beginning at very different points, all lead to neutral dialogue. But then, he concedes, they don't necessarily lead there either. The argument from "liberal skepticism," i.e., that "there is no moral meaning hidden in the bowels of the universe," seems to have priority over other of the avenues to dialogue: Ackerman calls this position "the hard truth" (p. 368). But, echoing Nietzsche, Ackerman also concedes: "If God is dead, everything is permitted" (p. 369). Is Ackerman then merely a bright and decent law professor attempting to paper over the unresolved problem of nihilism as proclaimed by Nietzsche?

MICHAEL P. ZUCKERT

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Against the Current: Essays in the History of Ideas. By Isaiah Berlin. Edited by Henry Hardy. (New York: Viking Press, 1980. Pp. liii + 394. $16.95.)

Those of us who do not save our *New York Review of Books* and who years later try vainly to cite details from what we remember as brilliant articles can be eternally grateful for this collection of Isaiah Berlin's essays. While several of them are old friends, the bulk of these 13 essays, having been originally published in much more obscure places, are new to most of us. What we have here is profoundly rich and rewarding. No student of modern political thought can read these essays without marveling at Berlin's ability to offer provocative and novel interpretations of writers like Machiavelli, Montesquieu, Marx, or Sorel. Whether one agrees with Berlin's own world view, with its skeptical, near tragically pessimistic variant of liberalism, or whether one agrees with his traditional history of ideas approach to the study of political thought, the reader of these essays stands in awe of their erudition, their range, and most significantly, their capacity to excite and to dazzle. Berlin is never boring or tedious. The reader is caught up in his prose and his arguments and wants only to have him there to agree with, to question, or to dispute.

It is hazardous to summarize a volume that contains so much and moves in so sprightly a way from the Italians Vico to Verdi or from the English Hume to Disraeli. But, surely, there is one thread that runs through most of these essays as it does through most of Berlin's writings. He is preoccupied with what Alfred Cobban years ago called "the revolt against the Eighteenth Century." Berlin is irresistibly drawn to write on those who repudiate enlightenment rationalism with its cosmopolitanism, its universalism, its optimism. While Berlin has characterized this revolt as a "German backlash against the French cultural domination of the Western World," he singles out as its first spokesmen a Frenchman and an Italian. Within the ranks of the *philosophes* themselves, Berlin sees Montesquieu subverting enlightenment dogma with his emphasis on the unique and the particular. The Italian, Vico, is much more central to Berlin's preoccupation, and several essays are devoted to him, emphasizing his rejection of a universal, immutable human nature, as well as the utopian belief in an ideal rational society. The "German backlash" is not neglected, however, for there are essays here on Hamann and Herder in which the heart of the anti-rationalist case is examined. While he is interested in their emphasis on spontaneous imagination, instinct, and insight, what most interests Berlin in their writings is nationalism.

Nationalism is, indeed, the aspect of the revolt against the Enlightenment that most fascinates Berlin and its understanding is at the heart of the best essays in this volume. Berlin moves from general speculations on the role of wounded pride in causing nations to exaggerate their own real or imagined virtues, to specific inquiry on the effects on Marxism of its systematic failure to appreciate the true nature of nationalism. Two essays in particular should be singled out in terms of the latter concern. There is a lovely piece on Moses Hess which helps us make sense of that enigmatic figure in the socialist tradition. More important, and indeed the most brilliant essay in the collection, is a discussion of Disraeli and Marx. Berlin links their writings and lives to their differing quests for identity as Jews. It is provocative, even outrageous, Berlin at his best, as he charts Disraeli's journey to the nation and aristocracy, and Marx's to the universal class and the proletariat, all as part of a desperate search for identity.

Few have written on romanticism and nationalism as well as Berlin has. One wonders, in fact, why his scholarship is so preoccupied with these themes. The answer may lie in Berlin's own skeptical liberalism, his vigorous denial in his other writings of the assumption in notions of "positive freedom" that there is a "final solution of the question of how men should live." Ultimately, Berlin's heroes are the "makers of pluralism." One such hero is Machiavelli who realizes "that ends equally ultimate, equally sacred, may contradict each other, that entire systems of value may come into collision without possibility of rational arbitration" (p. 74). Another hero is Herder, the critic of enlightenment hubris, who recognized the potential variety of cultures and nations, all offering equally ultimate paths to human fulfillment and none with a monopoly on reason or universal truth.

ISAAC KRAMNICK

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The Sexism of Social and Political Theory: Women and Reproduction from Plato to Nietzsche. Edited by Lorenne M. G. Clark and Lynda Lange. (Toronto: University of Toronto Press, 1979. Pp. viii + 141. $15.00, cloth; $5.00, paper.)

This edited collection contains essays on Plato, Locke, Rousseau, Hume, Hegel, Marx, and Nietzsche. It has the usual unevenness of edited collections but is useful both in substance and in clarifying an important methodological problem faced by any feminist re-reading of Western political thought.

Lorenne Clark and Lynda Lange state that each author has assumed, first, "that an adequate political theory must allow the same rights, duties, privileges, and liabilities to all persons regardless of gender" and second, "that reproductive labour is as socially necessary and humanly important as productive labour" (p. viii). This is the ground on which they assess the works of the theorists considered. The editors "hope that the essays in this collection show ample reason for concluding that traditional political theory is utterly bankrupt in the light of present perspectives" (p. xvii). This is too much to hope for from a series of essays, and the editors themselves add that what is needed is rather the development of theories which "reflect a deeper understanding of our historical position and a profoundly changed perception of sexual and reproductive reality" (p. xvii). The book as a whole indicates both the importance of a feminist critique of Western political thought and the complex theoretical repercussions of assumptions and arguments that women have no place in public life. The essays on Plato, Rousseau, Hegel, and Marx demonstrate the possible fruitfulness of approaching theorists from the perspective of the two assumptions the editors describe, while the essays on Hume and Locke illustrate some of the pitfalls of such an approach.

Lange's essay on Plato demonstrates that his recommendations for including women in the guardian class in *The Republic* do not "necessarily imply that *any* of them will become guardians" (p. 4). And her essay on Rousseau indicates that one cannot solve the problems posed by a theorist's exclusion of women from citizenship by simply reading the term "man" as "men and women." Mills points to the ways in which Hegel's assumptions about the nature and proper role of women involve him in circular reasoning (p. 90) and lead him to contradict "his own analysis of how one can achieve particularity" (p. 84). These non-obvious conclusions indicate some of the interesting theoretical results which follow from questions grounded on the assumptions of gender equality.

The essays on Locke and Hume, however, pose an important methodological question. Clark has a tendency to argue that to the extent that Locke held that (1) there is a natural inequality of the sexes, (2) that reproduction is not a central fact of political life, and (3) that the family is a " 'natural' institution" he can be accused of sexism (pp. 16-17). Hume is accused of sexism both because the double standard he supports implies that "men are the possessors of both property and children, and the only providers of goods from the world outside the home" (p. 56), and because his methodological commitments make it impossible to raise questions about sexist discrimination as a moral issue (p. 65). It is of course true that Locke and Hume hold such positions. But one is tempted to remark, "So what?" If one takes the kind of materialist position that most authors in this book seem to, one cannot expect the political theories of the past to share contemporary assumptions. The important question is not whether they share the assumptions of gender equality, but rather what role their treatment (or nontreatment) of women plays in their theories and what effects it has. The essays in this book outline several possibilities. The assumption of women's inferiority may (1) be consistent within the theory as a whole (Hume), (2) involve the theorist in circular or contradictory reasoning (Hegel), (3) undermine the strength of a theory as a whole (Rousseau), or (4) represent a deep lacuna in a theory which purports to be universal (Marx). This feminist reading of Western political thought should perhaps be more explicitly informed by Marx's argument that theorists can only confront the problems which history presents. O'Brien, despite her criticism of Marx for never coming to terms with the social relations of reproduction, argues that only very recently have issues of reproduction, and the possibility of rational control of reproduction (in addition to production), "become thinkable" (p. 113). While I am dubious of her location of this possibility in the technology of potentially universal contraception, her point that the writing of political theory takes place in a historical context indicates that to the extent that the tradition of Western political thought informs and justifies contemporary political practice, a careful study of the effects of what we now term sexist assumptions is essential. Despite the problems posed by some of the essays, this book makes a real contribution to this effort.

NANCY C. M. HARTSOCK

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Demand Revelation and the Provision of Public Goods. By Edward H. Clarke. (Cambridge, Mass.: Ballinger, 1980. Pp. xix + 237. $26.50.)

A decade ago two graduate students in economics (Edward Clarke and Theodore Groves, working independently) discovered what seemed to be a solution in principle to the famous "free-rider" problem of social choice. Part 1 of Clarke's book gives his own account of the "demand-revealing" (DR) mechanism and its significance; Part 2 is a discussion of how the practical difficulties of implementing DR might be handled.

This work came to wide attention only four years ago with the publication of lead articles in two of the most prestigious journals in economics (T. N. Tideman and G. Tullock, "A New and Superior Process for Making Social Choices," *Journal of Political Economy,* December, 1976; T. Groves and J. Ledyard, "Optimal Allocation of Public Goods: A Solution to the 'Free Rider' Problem," *Econometrica,* May, 1977). But Clarke's is already the second book-length exposition, following J. Green and J. J. Laffont, *Incentives in Public Decision-Making* (North Holland, 1979). In addition, there have been several compendia of papers; many individual articles; and at least two special issues of journals: *Public Choice* (Special Supplement, Spring, 1977) and *Review of Economic Studies* (April, 1979).

In terms of technical finesse, Clarke's book is not in the same class as the Green and Laffont book; but for political scientists interested in why economists have been so taken with the DR idea, Clarke's book will probably do better, and at almost half the price. However, my own perhaps overdeveloped sense of frugality leads me to recommend Dennis Mueller's *Public Choice* (Cambridge, 1979), which is a factor of two again cheaper and contains a good chapter on DR amidst a stimulating survey of the whole literature which attempts to apply economic theory to social choice. A (contrasting) view of DR, which I will sketch in a moment, is developed in a chapter of my *Selfishness, Altruism and Rationality* (Cambridge, forthcoming).

The problem that DR is intended to resolve was first formulated in a really clear way by Samuelson in an elegant three pages of the *Review of Economics and Statistics,* 1954. *If* we accept the distribution of income as fair (an "if" stressed by Samuelson but often ignored since), *then* the optimal level of supply for a public good will be where the summed (marginal) demand across all citizens just equals the marginal cost of the good. However, Samuelson argued that citizens would find it in their interest *not* to reveal their true preferences. Hence a political process would be required to make social choices. But what the proponents of DR believe they have shown is that, given (in Clarke's phrase, p. 110) "rational, egoistic, maximizing individuals with full information," it is possible to design a special tax (aside from an individual's normal tax, and paid only on the change in the social choice due to his own response) such that truthful demand-revelation is in the self-interest of each voter. What this amounts to is that it is possible to offer everyone an opportunity to buy a shift in the social choice with the cost (the special DR tax) arranged in such a way that we will be able to infer all voters' true demand from their responses to this incentive arrangement.

It is important to understand here that DR does not purport to overturn Samuelson's "free-rider" argument by changing his assumptions. To the contrary, Samuelson's own analysis is based on the same standard (within economic theory) behavioral assumptions. That is why Mueller can define the literature he surveys as the body of work which applies an axiom of rational egoism to politics.

Clarke's book mentions in passing the possibility that human beings may not actually be the egoistic maximizers of economic theory. But it mainly follows customary usage in treating this question as not really open to discussion. Hence Clarke's treatment of difficulties for DR is focused on problems relating to the artificiality of the "full information" assumption, asssuming the egoistic assumption to be a good enough approximation of human motivation.

However, it happens that DR provides an occasion on which it is especially simple to see that the customary assumptions about "economic man" are empirically indefensible. Clarke and other proponents of DR have pointed out that as the number of participants in a DR choice process increases, the incentive tax on a typical individual approaches zero. It turns out, for example, that for a DR choice in a country the size of the U.S. and a budget item on the order of $1 billion, the special-incentive tax on a typical voter would be on the order of 1/100,000th of a penny. This raises obvious practical problems: how would you collect such a tax, or reformulate DR in a way that the taxes would be small enough to be a plausible adjunct of the decision process, yet large enough to be meaningful to voters? It is concerns such as these that motivate Part 2 of Clarke's book and much else in the DR literature. But a more fundamental issue arises. Under DR, the effect of a vote depends on the size of the DR tax incurred. But DR proponents own calculations of plausible DR taxes for rational egoists are easily shown to imply that a voter willing to spend even a penny to increase the effect of his or her vote could increase that effect by a factor of 300. Then is it credible that a human being motivated to vote in the first place would not wish to spend even a penny to make that vote effective?

The difficulty here turns out to lie not in the assumption of rationality (internal consistency), or in any merely practical problems, but squarely in the assumption of egoistic motivation. Further, it is easy to show that if in fact real voters would spend a penny to increase vastly the effect of their vote, the demand information that would be elicited by DR *cannot* be the summable demand required to define efficient social choice.

Perhaps these results are not even very startling. The "economic man" model has never been able to provide a plausible account of the reason a person votes in the first place. Is it then any surprise that the same model of human choice also does not tell us much about how a person will vote, given that he or she votes?

HOWARD MARGOLIS

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The Practice of Political Authority: Authority and the Authoritative. By Richard E. Flathman. (Chicago: University of Chicago Press, 1980. Pp. xii + 274. $20.00.)

In this book, Richard Flathman offers an analysis of the concept of political authority and a justification for the practice of it. The analysis is intended to tell us what political authority is and is not. The justification is meant to show that the practice of political authority, correctly understood, is morally acceptable.

As a framework for his discussion, Flathman identifies two types of theories about authority, the substantive-procedural (S-P) type and the formal-procedural (F-P) type. The former seeks to identify, understand and assess authority "in terms of its alleged end or purpose. . ." (p. 2). Flathman finds the "supreme instance" of this type in "Plato's argument concerning the philosopher king in the *Republic*" (p. 16). Contemporary representatives of this approach are writers "whose thinking about politics is substantially influenced by religious considerations, and . . . social scientists of a functionalist bent" (p. 16). By contrast, according to F-P theories, "Authority has to do with form not function, procedure not purpose, antecedents and credentials not substantive content or consequences. Considerations of content or merit, of ends and purposes, are categorically irrelevant to understanding authority and to participating in authority relations" (pp. 14-15). "Plato's most celebrated competitor among F-P theories," Flathman says, "is in Hobbes' *Leviathan*" (p. 16). And the "best developed and most uncompromising [contemporary] version" (p. 5) of an F-P theory, according to Flathman, is Michael Oakeshott's.

Flathman contends that the F-P approach is the more promising for achieving an adequate analysis of the concept of authority, while the S-P approach affords a better basis for a moral justification of the practice of authority (to which justification only a little less than the last third of the book is devoted). However, he finds all the F-P analyses of authority he has encountered inadequate since they fail to give proper weight to considerations which are prominent in S-P theories. Contrary to the F-P analyses he considers, Flathman argues that (1) "teleological or consequentialist considerations . . . are internal to rule making and rule following. . ." (p. 6), and that (2) "in order for there to be rules that carry and bestow *authority* . . . there must be values and beliefs that have *authoritative* standing among the preponderance of those persons who subscribe to the authority of rules" (p. 6). Because these values and beliefs do not acquire their authoritative status as a result of promulgation or adoption by agencies with authority, F-P theories tend to exclude them from their analyses of authority, thereby rendering "incomprehensible the distinctive feature of authority, that is rules and conduct guided by rules" (p. 7).

In the course of working out his own analysis, Flathman discusses at length various conceptual features of rules and rule following, the problems of reconciling political authority and moral autonomy, and how authority differs from apparently related phenomena such as power and force. In his defense of the practice of authority, Flathman considers various political arrangements in which the tension between authority and autonomy would be minimized, and the ways in which political authority, properly constituted, would actually enhance the agency of those subject to it. Because in both the analytical and justificatory portions of the book Flathman works toward his own position by considering critically the views of other writers on the subject, the book includes a wealth of information about the literature on authority. In addition to Oakeshott's, the views of figures as diverse as Hannah Arendt, Peter Bachrach, Steven Lukes, Joseph Raz, G. E. L. Anscombe, Robert Paul Wolff, Ludwig Wittgenstein, Richard Friedman, Hobbes, Kant, and Rousseau receive more than passing attention.

Indeed, the chief value of the book is to the non-specialist for whom it affords an excellent entrée to the main problems regarding the concept and practice of authority, the principal approaches that have been taken to those problems, and much of the traditional and contemporary literature on the subject. However, the specialist who already knows this topic and its literature will have reason to doubt that the book advances the discussion substantially, primarily because of the way Flathman has construed some of the competing views on which his position purports to improve. The prime example is his treatment of Oakeshott's position, in which Flathman ferrets out crippling mistakes which, as one learns on a fairer reading of Oakeshott, are not there in the first place. As a result, Flathman's improved version of Oakeshott's theory turns out to be virtually equivalent in substance though not in style to the theory on which it was supposed to improve.

LEON GALIS

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Educative Democracy: John Stuart Mill on Education in Society. By F. W. Garforth. (New York: Oxford University Press, 1980. Pp. viii + 256. $27.50.)

One ought to have a good reason for writing a new book on a theorist about whom so much has already been written. F. W. Garforth has one, and his focus upon John Stuart Mill's ideas on education should provide us with a good reason for reading this book on Mill.

Unlike Locke, Rousseau and even Bentham, Mill did not write anything dealing exclusively with the education of children. Only on the occasion of delivering the Inaugural Address at the University of St. Andrews did he devote himself to the question of what constitutes a good liberal education. As Garforth's discussion indicates, Mill's definition of education helps explain why he had not previously written a work on education or, more accurately, why he must have believed that all of his previous works were devoted to that subject. According to Mill (here he follows his father), education embraces the whole environment of men and women; it encompasses whatever helps to shape the human being. "In its largest acceptation it comprehends even the indirect effects produced on character and on the human faculties by things of which the direct purposes are quite different; by laws, by forms of government, by the industrial arts, by modes of social life, nay, even by physical facts not dependent on human will, by climate, soil and local position" (p. 38).

In defining education so broadly, Mill runs the risk of neglecting to analyze carefully the nature and purposes of educational institutions more narrowly understood. Not surprisingly, therefore, he retreats to a narrower definition in his *Inaugural Address* and in his specific discussions of education in *On Liberty* and the *Principles*. There he identifies education with a set of formal institutions whereby older generations endeavor to hand down knowledge, culture and moral values to younger generations. However, Mill's broader definition also has an advantage: it shifts our focus to the educative potential of social institutions other than schools. In particular, Garforth emphasizes Mill's idea that the political institutions of a good society ought not only to legislate, execute and administer laws, but that they ought also to educate citizens. Mill argues that representative democracy is the only form of government suitable to political education in advanced societies like England and the United States. Unfortunately, Garforth misses the opportunity of evaluating Mill's reasons for supporting non-democratic rule in India and other less-developed countries. An "educative society" (Garforth's term) would be one in which all major social institutions, including the family, government and schools mutually reinforce five primary values that he attributes to Mill: happiness, individuality, truth, freedom and altruism. Or at least, the tension among the values fostered by these institutions should be a creative, rather than a destructive one, resulting in a "self-generating, self-nourishing education process which moves by its own momentum towards improvement" (p. 39).

Garforth realizes that there are obvious problems with this ideal, and he asks the most critical questions of it: "What values should be fed into the process? who will select them? may not the educative society degenerate into a totalitarian monster. . . ?" (p. 40). The answers that he attributes to Mill, however, are sometimes less than fully satisfactory in themselves or as interpretations of Mill's own views on the question. Ironically, given the claims of utilitarianism to be a practical theory which can resolve value conflicts, one strains to imagine the practical implications of this vision of an "educative society" devoted to the five values that Garforth credits to Mill. Garforth adds to this pantheon of values that "a primary function of education in any society is to promote the cohesiveness and continuity of that society." Yet he does not indicate how Mill might resolve the historically recurring conflict between those who argue that social cohesion is *the* primary function of education and those who propose above all to teach children how to think critically, to pursue truth or to discover the good life. Garforth quotes Mill's essay on Coleridge but fails to pursue the implications of Mill's claim there that the principles of individual freedom, political and social equality are "the only shape in which the feeling [of social unity] is likely to exist hereafter." Nor does he supply an answer to the second question, either in Mill's name or his own. Instead, he appreciates the tension within Mill's thought between his support for representative democracy and his desire for rule by the best. To answer the last question, Garforth offers Mill's view that the state ought to enforce but not to direct education (p. 123). Parents should be directly responsible for selecting a good education for their children and should be fined by the state if their children fail to meet minimum educational standards. Similarly, teachers should be held responsible for the results of their teaching. Garforth "excuses" Mill on historical grounds for the latter view (p. 141).

Perhaps the most critical and original educational claim Mill made was that only a representative democracy could be a form of self-education (pp. 178-80). It is a difficult claim to interpret literally, since if nothing else, democracy means collective rule by the people over one another. Taken seriously, therefore, this claim would push us in the direction of exploring Mill's understanding of the self and of the proper educative structure of democratic institutions—or else in the direction of criticizing his claims for representative democracy as a form of self-education. Garforth (rightly, I think) moves in the former direction, but the scope of his proposed enterprise combined with his frequent attempts also to account for Mill's ideas by referring to his personal life and his historical context prevent Garforth from reaching his proposed destination. Political theorists should still appreciate the fact that *Educative Democracy* reopens an extremely important question: What is the proper role of institutions other than schools in educating democratic citizens?

AMY GUTMANN

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Manipulatory Politics. By Robert E. Goodin. (New Haven, Conn.: Yale University Press, 1980. Pp. x + 250. $17.95.)

Manipulatory Politics offers the analytical political philosopher's half of an analytical political philosopher's conception of a collaborative effort with empirical political science to study a topic that a generation touched by Watergate and other political scandals will find timely, that is in keeping with the fundamental traditions of the discipline, and that "blends the empirical and the normative in a particularly constructive fashion" (p. 1). Manipulatory politics is a subset of power politics: power exercised (1) deceptively and (2) against the putative will of its objects. Goodin argues that "manipulation constitutes the evil core of the concept of 'power' . . ." (pp. 8-9).

The study is couched in rationalistic terms rather than psychological ones, "not because they point to a more morally important side of the phenomenon (they do not), but rather because they point to a side of the phenomenon more subject to our control" (p. 29). It purports to offer descriptions of casess that "are thick with both theory and detail" (p. 34) in the "preliminary task of cataloguing various modes of political manipulation and assessing them from a moral point of view" (p. ix).

The bulk of the work is devoted to 20 mechanisms of manipulation, grouped in six categories: untruth, linguistic traps, rhetorical tricks, symbolic rewards, rituals, and obviousness. Goodin asks a standard set of questions about each: (1) Is the interference deceptive? (2) Is it contrary to the putative will of those subject to it? (3) How persistent is the effect of the manipulation? and (4) Are any distributive biases inherent in it? The general pattern of his conclusions "indicates how very little cause for concern most of the tricks surveyed in Chapters 2 through 7 provide" (p. 233). He argues that most of them are not necessarily manipulative at all, and even when they are, their effects are often easily overcome.

This "broadly reassuring" conclusion is subject to an important qualification. Goodin's catalogue is "nowise exhaustive." He claims to have discovered in the rigging of the obvious "one very troubling form of political manipulation which has long gone unrecognized" (p. 236) and which is both "perverse and pervasive" (p. 36). And we do not know what other manipulatory tricks may be succeeding behind our backs. Rigging perceptions of obviousness shares with the two other "altogether worrisome" techniques (debasing language and magico-religious political rituals) three traits: they are distributively biased in favor of the powerful, they are to a large extent impersonal strategies of domination, and they tend to be self-perpetuating. The upshot of the argument is that we need to "focus attention first upon the biases of the political system" (p. 238).

The book thus offers three sorts of points of departure to empiricists: one might argue against Goodin's very impressionistically supported claim that most techniques of manipulation are not that important. One might study the three worrisome forms in a richer substantive context. And finally, one might accept his invitation to search for undiscovered forms of manipulation. All three sorts of study appear to be established firmly enough by Goodin's effort that they will not self-destruct, and the basic suggestion that we attend to biased, impersonal, and self-perpetuating traits of political systems seems wholly appropriate.

Although *Manipulatory Politics* may succeed on its own terms, several reservations must be entered. The choice of a "rationalistic" model is admitted to neglect the more important side of the topic. A perspective with more analytic (psycho-, not linguistic) depth would not neglect the rational, but rather set it in a much richer theoretical context, behavioristic prejudice notwithstanding. And Goodin's combination of analytic philosophy and empirical science, of the normative and the empirical, still retains a strong positivist flavor. It is disappointing that the past 20 years of innovative work in the history and philosophy of science should still have so little impact on our discipline. Analysis is not a substitute for theory.

JOHN R. CHAMPLIN

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Liberal Equality. By Amy Gutmann. (New York: Cambridge University Press, 1980. Pp. xi + 318. $34.95, cloth; $10.95, paper.)

As liberty was the predominant theme of much of nineteenth-century political theory, so equality has come to dominate the scene in the twentieth century. How significant is this shift? Amy Gutmann has set out to discover how far liberal principles can be pushed toward egalitarianism. She has done well. By "liberal principles" she means those that can be derived from an individual's interests; and by "egalitarianism" she appears to mean the belief that justice calls for movement toward greater equality than now prevails with respect both to material welfare and psychological satisfaction. She argues that liberal principles, properly extended, go further in that direction than is commonly recognized. But ultimately one reaches a dividing line between liberal and communal egalitarianism. The latter calls for a radical transformation of the moral consciousness. It depends on the proposition that "what constitutes an individual's interest cannot precede an understanding of what constitutes a desirable political community" (p. 219). Even the most extreme liberal egalitarianism, on the other hand, "does not require a complete remaking of moral consciousness" (p. 229) and assumes some knowledge of an individual's interest at the outset. The philosophical issue in the final analysis, she says, hinges on who must carry the burden of proof—an unsatisfying but arguably inescapable conclusion.

After an initial chapter on the classical liberal foundations, the least satisfactory of the book, she deals successively with various liberal thinkers and groups of thinkers: to wit., J. S. Mill, the Fabians, "the relevant reasons approach" (Bernard Williams and Michael Walzer), John Rawls, and Rawls' critics. A chapter (the last except for a conclusion) on "Participatory and Distributive Equality Reintegrated" draws together and amplifies her positive arguments as they have emerged in the preceding discussions. Here most fully her own theory is set forth. It entails "participatory democracy" as an end state but not as a first step.

The classical liberals all assume some kind of human equality: with Bentham and James Mill it is equality of passions—self-interest, with happiness as its measure; with Locke and Kant it is rationality. Perhaps she makes the distinction more simple than it is and compression has forced me to state it more simply than she does. Both assumptions face challenges (as to the facts and as to what normative conclusions would in any case follow), which she considers and finds less than sufficient. It is, however, Mill's participatory ideal and his openness to state intervention that are particularly relevant to her concern, for it is the tension between the redistributive and the participatory ideals of liberalism that she wishes to resolve.

After an interesting discussion of the Fabians, she turns her attention to what she calls the "relevant-reasons approach" to liberal egalitarianism. She finds that both Walzer and Williams fail to follow the most profound implications of Marx's view of the self in accounting for the values of liberty and community, and that because of this failure we are left with no guidance as to "how far beyond the welfare state liberal egalitarianism can rightly extend" (p. 118).

Among the many points that Gutmann makes in her discussion of Rawls, one is especially worthy of note: Rawls is weak in relating his theory of justice to democratic procedures. Gutmann would modify and, in her judgment, improve his theory by placing substantive justice, in the form of welfare rights, ahead of the procedural justice of equalizing participatory opportunities. This she would do, in part, to protect minorities. Welfare rights would be given constitutional status as would the Difference Principle. Accordingly, she believes that the first stage of a liberal program must be the achievement of consensus on the theory of justice. The failure of many recent attempts to implement participatory objectives is attributable to the lack of essential pre-conditions, of which consensus on principles of social justice is perhaps the most important. Although at one point she professes ignorance as to how a socially just society can be attained with justice, that problem would seem to disappear (perhaps in favor of a still larger one!) if consensus on the rules of justice must come first.

The reader is left facing an even tougher question: is the Rawls-Gutmann theory of social justice even theoretically possible, given the Humean constraints, which she accepts? Will most people work hard enough in a society in which they have to improve the lot of the least advantaged before they can benefit themselves? Will executives devote time to inventing tax-avoidance schemes at the expense of socially productive thought? Questions of moral psychology—how to obtain a consensus on justice and how great self-serving incentives must be—are crucial. But this comment is less a criticism of the author than it is a description of the (reasonable) limits she has set herself.

The book has a useful bibliography and extensive substantive footnotes.

J. ROLAND PENNOCK

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Philosophy and the State in France: The Renaissance to the Enlightenment. By Nannerl O. Keohane. (Princeton, N.J.: Princeton University Press, 1980. Pp. xii + 501. $30.00, cloth; $12.50, paper.)

The astonishing thing is the size of the gap which Nannerl Keohane has filled with *Philosophy and the State in France.* There are many studies of the Enlightenment, and especially of thinkers like Montesquieu and Rousseau; there is also much excellent scholarly work being done in the Anglo-Saxon world on Bodin and other political writers of the sixteenth century. Some of this work has filtered through into more general awareness. There is remarkably little on the French seventeenth century, however, and it is hard to think of any one who has turned into intellectual history the political ideas of the French in the three centuries before the Revolution. The very generosity of French writers such as Voltaire and Montesquieu in appreciating the virtues of the very different English political tradition has merely encouraged Anglo-Saxon parochiality, and the vast spread of interest in political ideas during the present century has done little to change the situation. Perhaps the cause is that Locke taught his countrymen to regard France as merely the source of a tyrannical absolutism; certainly the consequence has been a degree of blindness to the richness of what flowered when the *ancien régime* was at its height.

The most striking single feature of Keohane's treatment of the period is her emphasis on the tradition of *moralistes* from Rabelais and Montaigne onward. That she is enabled thus to extend the more familiar preoccupations of political theory results from the underlying schema used to help us focus the main features of French discussion in this period. The first and most familiar strand of thought is Absolutism which was distilled out of long-standing claims of the French monarchy and emerged with Bodin's theory of sovereignty. It came to be complemented in time with Richelieu's emphasis on *raison d'état.* In the rhetoric of Bossuet and the ambitions of the Sun King, this was the France which English Whigs found so terrifying they could barely distinguish it from the despotisms of Turkey or Russia. The second and perhaps less familiar strand was Constitutionalism which preserved France's mediaeval heritage of law and became "a distinctive blend of legalism, pluralism, and historicism" (p. 16). Keohane's third mode of argument is rather less conspicuously "given" in her materials, and she calls it Individualism. It "was not initially a way of thinking about politics so much as a philosophical exploration of the delights of privacy, based on a sharp separation between public and private life"

(p. 19). "The individual," she tells us, "made his appearance in French philosophy in the *Essays* of Montaigne."

The most striking thing about this individual was that he was developed out of disillusion and retreat from public affairs. The religious dissensions of sixteenth-century France encouraged many men of sensibility to retire from the brutalities and *bouleversements* of public life, often into philosophy. As with most developments in European thought, this exploration of individuality was strongly colored by the revival of classical doctrines, in this case Stoicism and, particularly, Epicureanism. Individualism resembled these classical doctrines in being an intellectual response to a world in which the individual seemed to have lost the capacity to influence events. Absolutism, apart from its historical roots in the ideology of the French monarchy, appears as a way to restoring peace to a nation ravaged for more than a century by strident sects and turbulent nobles. The internal emigration of many of Keohane's individualists is thus a response both to the disease and to the cure. There is, significantly, little like it in English thought, though it casts an illuminating light upon Hobbes to regard him as in some respects part of the province of French intellectuality. (His affinity, however, does not save him from being misunderstood as a believer in arbitrary despotism, as on occasion by Rousseau: Hobbes degenerates into Hobbism as Machiavelli into Machiavellianism.)

Thus it is that this most thoroughgoing exploration of the nature of modern individuality all took place under the aspect of a despairing realism, which imagined that men were steadily becoming more decadent and the world desperately lacking in classical virtues. Many French thinkers came to terms with religious dissension and commercialism, which initially bade fair to tear their society apart, by discovering that these things were based upon self-love; and then moving on to reflect that greed and the pursuit of selfish interests were, properly channelled, a useful form of social energy. The invisible hand, and the whole idea of the unintended consequences of human actions, were being explored in moral thought for more than a century before they finally flowered as the materials of a science of economics.

A concern with national wealth, however, was far from lacking through this period and already in the early seventeenth century the mercantilist idea of international trade as a zero-sum game was well established. In the *Testament* of Richelieu, particularly, we find a conception of the modern sovereign state as a bundle of resources, human and material, which must be bent to the direction of a single governing will. It is no doubt misleading to read into this doctrine the corruptions of later totalitarianism, but there is no doubt that all manner of despotic potentialities had been clearly grasped and that something altogether new and possibly sinister had appeared for the first time on the soil of Northern Europe. As Keohane is aware, the doctrine of *laissez-faire* which emerges out of mercantilism in this period had "more in common with the parent system than first meets the eye" (p. 158). She gives no warrant for the popular confusion of *laissez-faire* with liberalism.

Keohane writes in part to show that the seventeenth century in France, "far from being a wasteland in French political thought, was a pivotal and exciting period" (p. 13). A further preoccupation has been "to shed light on the political theories of Montesquieu and Rousseau, by showing how their arguments are rooted in earlier French thought" (p. 14). The result is to highlight, for example, Rousseau's enormous subtlety in analyzing the character of the individualist will in a modern society. Keohane sketches in the social and political background where necessary, but does not fuss about it. Her primary concern is with a set of ideas which are well worth attention even outside their social context, and there can be few students of modern political thought who will not find in the book much to be grateful for.

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Trust and Power: Two Works by Niklas Luhmann. By Niklas Luhmann. (New York: John Wiley, 1980. Pp. xix + 208. $28.95.)

Niklas Luhmann seeks to encompass major aspects of life in his book, and his ambitious intensity urges careful attention. *Inter alia,* he seeks to relate three theoretical approaches—system formation and differentiation, evolution, and an "as yet, tentative theory of symbolically generalized communication media" (p. 108).

Seven points may help orient the reader who takes on the tough task of tracking Luhmann's work. First, he is perhaps the most energetic "functionalist" since Parsons, although *Trust and Power* marks his first major exposure to English-speaking audiences.

Second, Luhmann is a theoretical dreadnaught who generates a turbulent wake. As Poggi gently notes in his very helpful introduction (p. viii): "Some reasons for the controversiality of Luhmann's work-in-progress may become apparent even from the following, brief presentation; others will perhaps suggest themselves to the critically-minded reader of the two works making up this volume."

Third, Luhmann reflects conviction about the promise of functionalism, as well as certainty about the failure of structural functionalism. This all but guarantees a range of critical attention. Those who loved Parsons' *Toward a General Theory of Action* will find much to critique in Luhmann's work even as they find some comfort in its gestalt; and those who could not abide Parsons' work may value Luhmann's revisionism even as they despair of his grounding in functionalism.

Somewhat more specifically, Luhmann insists that functionalism is a comparative method for relating "problems" to their range of alternative solutions. This contrasts sharply with the structural functionalist search for causal relationships. In Luhmann's words (p. ix): "The functional argument does not consist in proceeding from a previously ascertained performance to a corresponding need, thus justifying the existence of the performance. A logical equation only exists between a formulated problem of reference and the class of all possibilities of performance."

Fourth, Luhmann avoids encountering the fullness of these potentialities, e.g., how to define "a formulated problem of reference"? Parsons attempts a conceptual double-play: *the* problems are the ones associated with system maintenance, whose major parameters are specified as "functional prerequisites," action to meet which is normatively constrained. Luhmann seeks to avoid relying on values to provide direction. Rather, he fixates on *a* formulated problem of reference—what Poggi calls (p. x) "the problem of problems" for Luhmann—"complexity" and its reduction by differentiated systems of thought and action.

Fifth, I was not satisfied with Luhmann's "complexity," which gets only very general attention, and by design. As Luhmann explains (p. 6): "Complexity . . . has to be defined in very abstract terms [as] a distinction between system and environment and in terms of a system's potential for actualization. This concept then signifies a number of possibilities which are opened up through system formation."

Sixth, conceptual development aside, Luhmann's focus involves what seems a critical contradiction. Thus he continually extols the virtues of abstractness over concreteness, of contingent arrangements over those that are relatively fixed or determinate, and of "the class of all possibilities of performance." Yet he fixates on "complexity," his problem of problems, which apparent contradiction finesses the overpowering challenge of "all possibilities of performance." Having made that crucial value judgment, Luhmann otherwise urges that values are inefficient and ineffective devices for ordering complexity.

Seventh, reasonable readers also can entertain serious doubts about whether empirical analysis will be served by Luhmann's approach. For example, is his reliance on "abstractness" a matter of "evolutionary superiority," as Luhmann contends? Or does that convention simply distance analysts from the laborious selection of some manageable set of concepts-cum-operations that will permit valid and reliable descriptions of reality? Thus "trust" now gets measured in a score of major ways; moreover, "power" gets even more differentiated operational definition; and, understandably, research employing those concepts/operations generates diverse results. Luhmann acknowledges this crucial source of variance in results concerning his two conceptual targets in the volume being reviewed, but only to recommend neglect of that source of variance. Concerning the stuckness in research, he proposes (p. 108) that "to come to terms with its different meanings takes us no further—unless towards wariness and, in the end, resignation. In such circumstances it is not possible to proceed step by step; . . . instead, we must try to use more general concepts." Conceivably, at least, Luhmann may thus merely exacerbate a major problem as he seeks a way out of the inadequacies of structural functionalism.

ROBERT T. GOLEMBIEWSKI

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Karl Popper. By Anthony O'Hear. (Boston: Routledge and Kegan Paul, 1980. Pp. xii + 219. $25.00.)

Two problems of knowledge have puzzled philosophers for centuries. One, originating with Aristotle, is the infinite regress of justification. The other, elaborated by Hume, is justifying future events from events already experienced. Karl Popper's high standing in the philosophy of science is due to his innovative answers to the second, the problem of induction. His failure to resolve the first problem marks the limitations of his philosophy.

Anthony O'Hear provides an excellent critical guide to Popper's theories. Though finally rejecting Popper's basic system of thought, O'Hear judiciously discusses its defining ideas. We are able to see the lovely simplicity of Popper's solution to the problem of induction: replacing justificiation with refutation. From this replacement follows the view that a theory requiring more singular statements is better (more precise) because more falsifiable; and theories covering the same ground can be ranked according to their verisimilitude (truth content over falsity content). Science thus avoids justifying theories, even on the strongest of empirical regularities. The scientist is concerned only with falsifying claims on the future.

O'Hear discusses at length the well-known rivals and objections to Popper's falsificationist thesis. The first, developed by Lakatos, is that any theory's failure—even in a crucial test—can always be explained by additional hypotheses. Since there is no theoretical limit to the chain of saving hypotheses, no falsification can occur. Popper's attempt to answer this objection—the most serious for his philosophy of science—with a demarcation between critical and uncritical approaches to knowledge and an emphasis on background knowledge does not quite overcome Lakatos' critique. We are inclined to favor Lakatos' acceptance of a protective belt of statements in science that are insulated from falsification. But the discussion generated by these clashing views—on the metaphysical or dogmatic status of research programs, the problems of crucial experiments—still enrich the literature. O'Hear's treatment, though not comprehensive (more on Lakatos' alternatives would have been helpful), is accurate and helpful.

The second objection is that falsification cannot address probability statements, for no event can falsify a probability estimate. Here Popper is at his best and worst. The solution: statistical stability is the result of propensities in the circumstances in which events occur; and the deviation of events from a stipulated tolerance is counter-evidence for the estimate. O'Hear points out both the failures of this answer—its reduction to a frequency theory of probability—and Popper's misapplication of his reconstruction to the two-slit experiments in physics (though O'Hear's discussion does not include Popper's technical contributions here).

The third objection—the one most familiar to social scientists—is the restoration of relativism by Kuhn and others and the attendant claim that incommensurability makes choices among theories necessarily arbitrary. The easy dismissal of Kuhn's separate worlds (a dismissal needed even for Kuhn's study) should be required reading for social scientists. Unfortunately for Popper, any encounter with relativism raises the Aristotelian problem of infinite regress. Aristotle distinguished basic from demonstrative knowledge. The former is true without evidence or justification. Popper uses basic statements, arbitrary stops in a regress. O'Hear outlines the main arguments against Popper's consensual support for basic statements: the differences between appealing to a rule and head-counting, the more general denial (by Wittgenstein) of a private language, the non-evidentiary status of some statements (the use of Austin would have been helpful here). Generally, a sketch is provided of alternative philosophies that supplant Popper precisely on the failures of his skepticism.

Karl Popper is one of the important philosophers of this century. O'Hear's book captures both the successes and the failures of his work. The discussion of fallibilism in political thought is perhaps the weakest part of the book—but perhaps this is also Popper's weakest contribution. O'Hear's exposition of Popper's Darwinism, the three-world theory, Popper's intriguing use of the Tristran Shandy paradox, the Oedipus effect, and Gödel's theorem to deny complete description and, subsequently, prediction—are well done. O'Hear has written possibly the best examination to date of Popper's overall philosophy.

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Authority. By Richard Sennett. (New York: Alfred A. Knopf, 1980. Pp. 206. $10.00.)

The concept of authority is no doubt central to the works of political theorists. It has also been a concern of a variety of social scientists who, in recent years, have examined American government, political economy, ideology, and culture, and have written about America's crisis of authority and/or crisis of legitimacy. Most often, these analyses begin with Weber's assumption that authority and legitimacy are intertwined, that authority is legitimate power. Hence, a crisis of authority is at the same time a crisis of legitimacy, and vice versa. Richard Sennett, however, stands Weber on his head: his claim is that in modern society authority is *illegitimate* power. There is, he argues, a crisis of legitimacy in modern political and cultural life which maintains forms of authority which people believe are destructive.

The central insight in the book is that human beings create authority, images of strength and weakness, and its meanings. Sennett examines how we feel and experience authority and interpret it into being. He concludes that Americans lack a sense of their power to imagine new forms and meanings of public authority and that, as a result, authority tends to be understood as total, permanent, and external to the self. Prevailing forms of authority we perceive as deceitful and unworthy of our trust, even as we remain psychologically dependent on them. Our fear and rejection of authority actually tie us more closely to and reinforce the power of images of authority we regard as negative. Having lost "a sense of some humane value of power" (p. 82) and still having a need to believe, Americans feel attracted to strong figures of authority even as they doubt their legitimacy and the legitimacy of the institutions they seek to govern. Sennett's analysis suggests—but only suggests—how advanced societies can experience a crisis of legitimacy and, at the same time and without new efforts at repression, produce no significant movement for social and political change.

For Sennett, there are two dominant images of authority in modern society. Both are rooted in organizational life. First, the paternalistic authority of bureaucratic organizations, like that of nineteenth-century capitalists, is a false love which promises care, but delivers only when it is in the interests of the masters and when those who are dependent remain passively quiescent. If a deceitful paternalism has created a culture of disbelief in all forms of authority, then the image of personal autonomy as authority has further privatized modern culture. Operating on "the principles of indifference to others and self-sufficing expertise" (p. 131), the autonomous figure arouses feelings of shame and dependence and offers an illusory view of freedom to subordinates. Seeing autonomy as a way to be free under unequal conditions, subordinates seek to defend themselves against the world of power and to find freedom and self-fulfillment in private life. Sennett, following Tocqueville, claims that this privatization of experience impoverishes public life and opens up the possibility of vast powers for a state which is a soft tyranny. A public realm of freedom, the work of imagining authority as legitimate, has been eroded in modern society.

The problem, then, is not that there is a crisis of authority in American society, but rather that there isn't. Sennett proposes that people need to create frequent crises of authority, to link authority and disorder, so that they will come to have a sense of the limits of public power and the ability to make and remake its terms and create the conditions of their own freedom. In short, democratic forms of authority-making will enable people to limit and respect authority. While Sennett indicates how this might be done in private life and, to a lesser extent, in organizational life he leaves us with only scattered hints on how this can be accomplished in the polity.

For the most part, the guide is Hegel and the unhappy consciousness. Sennett probes the dialectical relationship between master and bondsman to show how people can imagine into existence new forms of power in private life which are not omnipotent, fearful, and mystifying. He suggests that if productive conflict can end psychological dependence on authority in private life, then a strategy of democratically deforming authority can achieve the same end in modern bureaucratic organizations. Subordinates must disrupt the universalism of paternalistic and autonomous authority and initiate an ongoing process in which authorities are clear about their actions and subjects continually negotiate over and decide about the meaning of power in their lives. Authority can be legitimized in democratic organizations where conflict is stuctured so that subordinates lose their shame and interpret and reinterpret the meaning of power—by locating the source of all orders and treating them as propositions to be tested, discussing the categories to which the rules apply, encouraging a variety of obedient responses to rules, role exchange which will suggest the limits of power, and face-to-face negotiation over the need for nurturance and support.

This is a wise and provocative book. Its analysis of the psychology of authority in modern society is excellent. What Sennett has not given us, however, is a full analysis of how and why his strategy of democratic deformation will work. He is most suggestive when discussing organizational life, although nowhere is it indicated why masters need or will respond favorably. Further, Sennett's strategy is only weakly tied to political life. There is an interesting passage about the need to break the link between the suffering and moral claims of victims and the reinforcement of the prevailing psychology of authority and power of managers. His discussion about the ways in which authorities can rightly be perceived as not having complete control is helpful. But there is little mention of the contemporary social and organizational conditions which apparently serve to reinforce a convenient conspiracy between masters and subordinates. Where is it written that the powerful will be responsive to democracy and that the subordinates will want their freedom? At some point in his truly impressive works, Sennett needs to analyze why this is politically—not just theoretically—possible.

CHARLES E. ELLISON

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Karl Marx and the Anarchists. By Paul Thomas. (Boston: Routledge & Kegan Paul, 1980. Pp. ix + 406. $40.00.)

The history of ideas can be done in a number of ways. One is to look at writings that hitherto have gone unnoticed. Another is to draw new (and perhaps surprising) connections between thinkers and theories. In his introduction Paul Thomas indicates that he will be doing both. The subject of his book is Marx's "oddly neglected" responses to his anarchist opponents—Stirner, Proudhon and Bakunin—a study of which, Thomas believes, will lead to a deeper understanding of both Marx's theory and the nature of politics. But in the course of exploring these neglected disputations, Thomas wants to establish a fairly striking thesis: whereas Marx is a descendant of Rousseau and Hegel, he maintains, the anarchists are the children of the *philosophes* of the Enlightenment. This revisionist genealogy is fundamental to Thomas' enterprise as it provides the basis for his central cleavage between Marx and the anarchists. While the latter pursue the logic of the Enlightenment's distrust of authority and politics to its limit, Marx sees revolutionary change as a creative and essentially political endeavor.

Thomas pursues the Hegel-Marx connection at great length, occasionally bringing in Rousseau as well. The core of this tradition is the distinction between state and civil society which, according to Thomas, Marx sees at the root of *political* alienation. In modern social orders, civil society is separated and isolated from the political, providing a distorted, "alien," political life. The opposition between public and private life, requiring as it does the splitting of personality into discontinuous roles, is thus "the measure of alien politics" (p. 121). Now, Thomas argues, Marx saw emancipation from such alienation to be in large part a political task, involving a transformation of both civil society and polity. But at this point a fundamental ambiguity enters into Thomas' account, for on the face of it this sort of "political" action would take place within the "bourgeois state" with an eye to its destruction. But Thomas adamantly insists that Marxian politics does not require a state, and that "the solution to alien politics cannot be no politics, but only politics of a different kind; it cannot be further depoliticalization, but only a repoliticalization of the required type" (p. 350). But one never gets a clear idea of the nature of this second sort of politics and its relation to revolutionary political action within the "bourgeois state." (Surely they must be different.) Admittedly, Marx may not have said much about "post-revolutionary politics," but if Thomas wants to argue that "political" is a critical category in Marx's thought—as he certainly does—then we need to get much clearer about the distinction between these two senses of "political."

In contrast to the Marxist genealogy, the anarchists' links to the Enlightenment pretty much drop out for the remainder of the book (except for a short reappearance in the conclusion). One can certainly understand why that family tree wouldn't be pursued too far: showing the communitarian, romantic, anti-scientific Bakunin to be any sort of descendant of the *philosophes* (and not Rousseau) isn't an inviting task. Moreover, of the few connections that do surface in the body of the book, Thomas' insistence that anarchists steadfastly maintain a "liberal," negative, conception of liberty is less than totally convincing, especially in light of Bakunin's claim that freedom has both negative and positive aspects (*Bakunin on Anarchy,* ed. Sam Dolgoff, Random, 1971, pp. 234-42). But though Thomas does not pursue this part of the genealogical thesis very wholeheartedly, he does constantly stress the anarchists' fixation upon the "political." Anarchists, we are told, detest the political realm because they are so obsessed with it. In contrast to Marx, their diagnosis is not premised on the alienating bifurcation of man into social and political selves, but the mere existence of authority, politics and the state. And thus they are said to both overemphasize and underemphasize the political. They overestimate what the state's destruction alone can bring about, while they underestimate (indeed are blind to) the value of liberal-democratic reforms as first steps to "human emancipation" (pp. 239-42).

It isn't really surprising that Thomas develops the Marx side of the genealogical thesis so much more thoroughly than the anarchist side, for this book is very much about Marx and his disputes with the anarchists. And because Marx did not dispute with Kropotkin, perhaps the greatest anarchist theoretician, the relation between Marxism and anarchism remains unclear. Thomas' insistence that means cannot be separated from ends (p. 342) notwithstanding, it certainly seems that anarchists like Bakunin and Kropotkin shared Marx's central idea of an integration and intensification of individuality and community (cf. pp. 9, 52). If so, then although Thomas has demonstrated fundamental differences in the two diagnoses from which their "tactical" divergence stems, the rift dividing them is still not quite so deep as Thomas would have us believe.

GERALD F. GAUS

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A Discourse on Property: John Locke and His Adversaries. By James Tully. (New York: Cambridge University Press, 1980. Pp. xiv + 194. $22.50.)

The "major aim" of *A Discourse on Property* "is to recover the meaning which John Locke intended to convey in his theory of property in the *Two Treatises of Government.*" As a student of Skinner and Dunn and the "new history" of political theory, James Tully argues that the means for accomplishing this objective involve, on the one hand, an exploration of the "normative vocabulary and conventions [of] seventeenth-century natural law and natural rights 'discourse' to which Locke is a contributor," and, on the other, a consideration of the social issues and political action to which Locke addresses himself in the *Two Treatises* (p. ix). *A Discourse* achieves a notable success in fulfilling these promises, especially with respect to the first set of issues.

This is a thoughtful, carefully reasoned, and tightly argued little book. Indeed, some readers may find the author's painstaking examination of the shifts and turns in natural law reasoning a bit overdone; nevertheless, there can be no doubt that it is both a serious reading of Locke's argument and an admirable contribution to the "new history" of political theory. I emphasize this point here because the real danger with respect to Tully's *Discourse* is that the intricacy of its arguments is likely to obscure the importance of its radical conclusions, and it is upon the latter that I therefore propose to focus my attention in this review.

Tully begins by establishing the basic framework for viewing Locke's treatment of property in the *Two Treatises,* which he suggests lies in a "workmanship model" derived from the theory of creationism. Essentially, this means that the maker of something has a right over and knowledge of that which he makes. Although this may sound a bit odd at first, it will easily be seen how it can account for man's ontological status as "the workmanship" of the Creator, his theological relationship to God his maker, as well as for the epistemological constraints Locke imposes upon men with respect to their knowledge of "real substances" which they have not made. That "the workmanship model is a fundamental feature of all Locke's writing," and constitutes "the ground of property relations as well as of many political relations" is convincingly demonstrated by Tully (p. 4).

In addition to placing Locke's thought within a traditional theological perspective, Tully argues that "Locke never considers a congeries of presocial and isolated individuals." On the contrary, "Locke's analysis always presupposes men organized into a unified community," as an "irreducible datum of man's existence" (p. 49). From the standpoint of historical research, the atomistic interpretation of Locke has never had much credibility, being an anachronistic application of a later view to seventeenth-century thinkers. By examining in detail the issues in the contemporary debate over natural law, Tully is able to show why Locke so frequently allied himself with the traditional views of Aquinas and Hooker. It is only when these Aristotelian dimensions of Locke's thought—his insistence upon beginning with individuals in families and organized communities under natural-law obligations to each other and to God—that his truly "radical intentions" (p. 89), as Tully calls them, can be appreciated.

"Locke's main ideological conclusion," he argues, is "that fixed property in land does not have a natural foundation" (pp. 123, 146). Whatever land an individual "enclosed and . . . tilled and reaped" in the state of nature, Locke wrote, became by "his peculiar right" his property. "But if either the grass of his enclosure rotted on the ground or the fruit of his planting perished without gathering . . . this part of the Earth, *notwithstanding his enclosure,* was still to be looked on as waste, and might be the possession of any other" (*Second Treatise,* par. 38). Tully develops the implications of Locke's account of property to show that an individual has "no right in land as such, but only a use right in improved land conditional upon the use of its products" (pp. 122-23). Locke's model of property ownership is the English Commons (pp. 124, 129, 135). "The only form of property in land which he endorses in the *Two Treatises* is the English Common." Moreover, Locke mounts a radical attack on primogeniture, and his "undermining of primogeniture clearly would have the effect of redistributing landed property into much smaller estates" (p. 169). In ideological terms, therefore, "Locke's theory serves explicitly to legitimate the rights of the commoners against the enclosing landlords" (p. 154).

Through a consideration of the interrelationships between the Lockean notions of "person," "liberty," and "property," Tully integrates the *Two Treatises'* discussion of property into a political theory in which "revolution against arbitrary government is legitimated" (pp. 130, 146). This aspect of the author's argument is less well developed in *A Discourse.* But there are indications of the direction in which it could be pursued. Tully notes Locke's assumption that laborers and tradesmen own their own tools (p. 140). Against Macpherson's conception of the bourgeoisie, he intimates that an "economic organization of skilled workmen" may be closer to the socioeconomic paradigm of Lockean thought (pp. 136, 143). In other words, the social audience for whom the *Two Treatises* was written was largely composed of artisans, tradesmen, independent yeomen, and small gentry. Hence the radicalism of Locke's political message.

We ought, therefore, to pay greater attention to the political and intellectual linkage between Locke's political theory and the position developed by the Levellers a generation earlier. Locke's treatment of property, according to Tully, incorporates most of the points at issue in the army debates at Putney between Ireton and Colonel Rainborough, with Locke siding with the latter (pp. 169, 174). In fact, Tully affirms Locke's belief in adult male suffrage, although the evidence for this is extremely thin (p. 173). Tully may be right, but he has a tendency to wander from the "historical" pathways of his argument, claiming, for example, that "it cannot be seriously doubted" that Locke read Aquinas, Suarez, or Cumberland (pp. 6, 65-66), when there is no hard evidence to support any of these claims. Certainly, every interpretation must have some inferences, but their overuse will soon exhaust the author's credit with the reader, leaving no basis for distinguishing between "recovering" a theorist's intentions and an interpreter's arbitrary assignment of intentions to him.

Nevertheless, *A Discourse on Property* presents a serious case for revising our thinking about Locke's attitude towards property, and the role played by his discussion of that institution in the context of the political argument of the *Two Treatises.*

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American Politics

Before the Convention: Strategies and Choices in Presidential Nomination Campaigns. By John H. Aldrich. (Chicago: University of Chicago Press, 1980. Pp. xiv + 257. $25.00, cloth; $7.95, paper.)

Before the Convention is a self-consciously theoretical treatment of the American presidential nominating process, based largely on an analysis of the Republican and Democratic contests of 1976. It represents a significant advance in the theoretical level of the study of the nominating process, and it further represents a desirable trend in which some of the profession's most sophisticated formal modelers confront data and do theoretircally informed empirical work.

The intellectual structure of the book comes from rational choice theory, based on the assumptions that candidates are goal-oriented expected utility maximizers, and that voter behavior can be explained in similar terms. There are two main sections of the book. Chapters 2 to 4 review the familiar background topics of who runs for president and why, the institutional context, and citizen participation and choice. While the topics are familiar, Aldrich sheds new light on them. The most original parts of the book, however, are in chapters 5 to 7, where Aldrich deals with the strategic problems of resource allocation and ensuing campaign dynamics, the choice of states in which candidates compete, and competition in policy space.

Among the strengths of the book are an operationalization of how willing political candidates are to take risks, and an enlightening discussion of the tradeoff between choosing candidates who are most preferred and candidates who are electable. Aldrich also develops an innovative and useful measure of candidates' competitive standing, based on the proportion of delegates won relative to the number yet to be selected. He develops and tests propositions regarding candidate choices of states in which to compete, and analyzes candidates' policy positions and issue emphases.

The most important part of the book is probably that dealing with the dynamics of campaigns. Performance in campaigns is based on resources available to candidates and measured against expectations. Performance in excess of expectations generates new resources and new expectations and vice versa. Aldrich works out the logic of these dynamics for two-candidate and multi-candidate contests. The mathematical basis is developed formally in an article in the September 1980 issue of this *Review*. The substantive treatment of the dynamics revolves around the question of stability.

Aldrich's basic prediction is instability in both two- and multi-candidate races, where instability is defined to include oscillation about a point of equilibrium in two-candidate races, and to include an inexorable rise to victory along with winnowing out opponents in multi-candidate races. The situation in which an initial front-runner continues in the lead and wins the nomination is clearly ruled out.

Thus the Aldrich instability theme is diametrically at odds with the Keech-Matthews theme in *The Party's Choice* (Brookings, 1976), that early front-runners often exist, and when they do, they usually win. Of course, that observation was wrong for the 1972 out-party nomination and irrelevant (in the absence of a clear front-runner) for the 1976 out-party case. Ironically, in the year in which Aldrich's much more theoretically sophisticated book appears predicting instability, early front-runners won nominations of both major parties.

Of course a changing context can help explain changes in the pattern of outcomes, and here the Aldrich book is again superior. While *The Party's Choice* underplayed the significance of recent reforms and rules changes, emphasizing continuities, Aldrich rightly points out that these new developments have strengthened the dynamic forces and the forces of instability in current campaigns. Yet the brief section relating the findings to previous campaigns is one of the least satisfying in the book.

While Aldrich's book has a strong theoretical base, its readability and its use to a broad audience is not hampered by its theoretical character. In fact, the book is strong on descriptive material regarding the 1976 campaigns. And the presentation of this material in an analytical context makes it a rather painless way to learn some rational choice theory and to see its relevance to a very concrete problem. This is an important book about presidential nominating politics, representing only a portion of Aldrich's contributions to our understanding of this subject. Its utility extends from the specialist to the general reader and the undergraduate.

WILLIAM R. KEECH

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The Reserve Mining Controversy: Science, Technology, and Environmental Quality. By Robert V. Bartlett. (Bloomington: Indiana University Press, 1980. Pp. xi + 293. $17.50.)

Scholars of public opinion consistently note that the American public can become politically motivated as a result of certain societal events. Such infrequent public interest has consequences for the policy process. Robert V. Bartlett describes and analyzes the nature of such consequences for the policy process resulting from a widespread change in social values. More specifically, Bartlett focuses on the controversy surrounding the Reserve Mining Company of Silver Bay, Minnesota, a controversy that well illustrates the policy consequences of a major public concern for the environment.

This study, however, goes beyond a description of specific conditions surrounding charges that Reserve Mining Company's policy of dumping millions of tons of mining wastes into Lake Superior resulted in aesthetic as well as health damages to the surrounding area. A broader question is addressed: "Is it possible then, for a society to obtain simultaneously the benefits of industrial technology and of a high quality national environment?" (p. 234). Although this question is not, and cannot be, answered at this time, Bartlett does provide more evidence of the conflictual nature of a policy process attempting to reconcile two interrelated, yet often contradictory, forces: industrial technological advances and attempts to protect the environment. The study shows that prior to this time of national concern for the environment, development of industrial technologies represented a major goal of society. What to do with the external effects of such development remained a secondary consideration. It therefore came as a major surprise to industry when the public demanded that the environment be cleaned up, and government responded to those demands.

The Reserve Mining Controversy vividly illustrates various characteristics associated with policy conflicts resulting from environmental concerns. In the first place, whether an industry pollutes the air or the water, thereby affecting those living in the surrounding area, requires scientific evidence that may or may not be available at that time. Yet when the public generates enough pressure, policy makers tend to react as a way of placating public demands. The ultimate decision, however frustrating it may be to those presenting scientific evidence, will be political in nature. In short, a course of action for a complex, technical issue will be largely based on subjective definitions of what that issue is and what should be done about it. In fact, Reserve Mining Company was ultimately forced to invest millions of dollars for an on-land dumping site even though no scientific evidence existed to prove that the health of citizens in the Lake Superior area was in immediate danger.

Second, this controversy nicely illustrates the federal nature of policy making in this country. A multitude of actors and institutions at all levels of government interacted continuously over the 12-year period to develop a plan of action. At a minimum, three states, several communities within these states, as well as the federal government, became involved in this controversy, not to mention the various private groups representing industry and environmental positions. Allowing so many access points to individuals and groups perceived to be affected by industry's actions leads to lengthy negotiation and efforts to find compromises. It is no wonder that it took 12 years to develop an acceptable program.

Third, Bartlett's study points to the changing nature of problem definition and the impact of such problem definition on policy deliberations. More specifically, the event that significantly altered policy debate in this case occurred with "aesthetic" considerations were replaced by "health" considerations. As soon as the possible presence of asbestos was linked to waste dumpings, and the possibility arose that the drinking water in the area was contaminated, the policy context changed dramatically. Such a redefinition led to public concerns that had not existed before, since more people were now perceiving this problem as affecting them directly. This pressure, in turn, forced policy makers to be firm with the Reserve Mining Company. It is unlikely that the federal courts would have required on-land disposal sites on purely "aesthetic" grounds. Only after health considerations became the center of controversy did it become clear to all parties involved that drastic action, with or without conclusive scientific evidence, would be required.

Bartlett presents a very detailed, well-researched case study, one that certainly adds to our disciplinary knowledge. Policy-related courses would benefit from this research as a supplementary source of information and analysis.

DIETER MATTHES

Francis Marion College

The Army Gets an Air Force: Tactics of Insurgent Bureaucratic Politics. By Frederick A. Bergerson. (Baltimore: Johns Hopkins University Press, 1980. Pp. xii + 216. $14.00.)

Foreign scholars are occasionally perplexed or amused by the attention given to bureaucratic politics by American political scientists. But it is clear that the practice and study of this phenomenon have been nourished by our distinctive social structure and ideological tradition. American egalitarianism encourages governmental processes which are open enough to enable outsiders to describe them in detail. At the same time, it develops enough entrepreneurial initiative in public officials to guarantee that the detail will be delicious. A direct line runs from Lincoln Steffens through early exponents of the case method like E. Pendleton Herring and Harold Stein to contemporary practitioners of the bureaucratic politics approach.

Frederick Bergerson's book, which won a Leonard D. White prize, is a high-quality, thoroughly researched example of what that approach can accomplish. His topic is the revival of an army air capability that had been all but extinguished after the creation of an independent air force. He attributes this rebirth to "bureaucratic insurgency" a species of "social movement" that develops within large-scale organizations.

In the struggle for an army air capability, the insurgents were a loose fraternity of middle-ranking officers and technicians held together by a common conviction that victory in combat was in jeopardy because the air force placed too low a priority on close air support of land operations.

Bergerson's time frame runs from 1941 to 1970. Initially army aviators were used only for artillery targeting and unit liaison, later for medical evacuation and the transport of cargo and infantry, still later to operate gun platforms. But the victorious insurgents then developed a fixation on technology akin to that of their air force rivals, and overreached by requesting the Cheyenne. This high-speed, high-cost assault helicopter was rejected by higher authority as needlessly sophisticated for most Third-World environments while insufficiently lethal or survivable for NATO.

Yet the insurgents had come a long way. Why? Bergerson supplies several reasons. One was technological: the advent of the helicopter. A second was the acumen of the insurgents. As specialized "fly boys" unlikely to make general, they sought allies among credentialed generalists destined for high command. A third was the strategy of the opposition. The air force sought to limit army aviation to logistics and communications, to confine it to a 60-100 mile battlefield zone, to restrict the weight of fixed-wing planes to 5,000 pounds, etc. At the same time, it neglected its own responsibility for close combat support because its leaders felt that their organization's "essence," to use Morton Halperin's term, lay in strategic bombing, that is, in ball-carrying, not in blocking or tackling for others. Moreover, the air force ignored the army's challenge too long. Bureaucratic insurgencies, Bergerson claims, are most easily put down at the outset.

So much for the tale itself. Of more widespread interest is Bergerson's concept of insurgency. While the term has a harsher connotation in common speech, he uses it to denote a rather moderate reaction in a spectrum of possible responses (vocal protest, slowdown, resignation, insubordination, mutiny) by individuals who disagree with organizational policy. The insurgent is an activist reformer, but not an outlaw. He remains loyal to the organization and its mission. Indeed, he feels that new policy is required for the good of the organization and the attainment of its mission. But he is not set on storming heaven. Like a missionary, he works gradually but persistently to convert the heathen. At the same time he takes advantage of loopholes, skirts the edge of regulations, evades formal orders, and is less than fully compliant when he cannot ignore them. Bergerson is aware of the normative implications of even such modest finagling. The problem is that challenges to hierarchical authority are also challenges to civilian control and thus to democracy itself. Against this, however, he sets the danger of an unchallenged, hence stolid, plodding, ponderous bureaucracy.

Since all of this is unexceptionable, let me add a few comments on the empirical side. One is that the degree of pluralism evident in American aviation—commercial as well as military—is sustained not only by the underlying social forces noted earlier but also by the wealth of the nation. No other country can afford four air forces. Pluralism is also more likely in the absence of peril. Conversely, central planning and control of aviation are more likely when the state itself is in jeopardy. Finally, just as technology made army aviation possible, so it may make it obsolete. If so, Bergerson's insurgents will prove to be transitory historical figures, though insurgencies of other kinds will recur.

LAURENCE I. RADWAY

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Representative Democracy: Public Policy and Midwestern Legislatures in the Late Nineteenth Century. By Ballard C. Campbell. (Cambridge: Harvard University Press, 1980. Pp. xi + 260. $20.00.)

In recent years political scientists interested in Congress have benefited from a number of studies of nineteenth- and early twentieth-century congresses that have provided valuable historical perspective. Now students of state legislatures (and particularly those interested in voting patterns in the legislature) can benefit from a study of state legislatures in the late nineteenth century.

This book by a historian examines the lower house of legislatures in Illinois, Iowa, and Wisconsin for the period 1886-1895, a period of socioeconomic change, considerable political ferment, and higher levels of party competition than in the past. Ballard Campbell combines the tools of historical analysis with those techniques used by political scientists—particularly roll-call analysis. The study is heavily dependent on roll-call analysis, which, while relatively simple, is adequate for the author's substantive analysis, which focuses on the importance of political party alignments and on major areas of public policy.

A few of the substantive findings can be summarized. The Democratic and Republican legislative delegations in each of the three states wre distinguished primarily by the ethnic and religious composition of the constituencies (and not by their urban or rural character). The individual legislators for the most part shared the dominant characteristics of the district. (Irish Catholic districts then, as now, produced Irish Catholic legislators.) Political party alignments appeared on a limited number of issues, but when party was not important, no other variables (such as urban-rural differences) proved to be consistently useful in explaining voting patterns.

The issues that created the strongest, and most frequent partisan divisions were those labeled "community mores" by the author. The most frequent of these was the regulation or prohibition of liquor; other issues involved educational policy, regulation of social behavior, individual rights, and nationalism. These were the issues on which the religious and ethnic groups that dominated the two political parties had sharply different views. Certain governmental issues such as the political structure or elections, in which the parties had a vested interest, also produced party voting—as would be expected. Most economic issues, however, did not produce strong, consistent party alignments.

The findings of this study are remarkably similar to modern analyses of voting in state legislatures conducted by political scientists, although Campbell pays little attention to the comparison. Then as now there were partisan voting alignments on those issues that were most salient to, and that distinguished, the groups and interests that regularly voted Democratic and those that voted Republican. Then as now the maverick legislators were ones who represented districts that were untypical for that party—for example, the Yankee Protestant district that elected a Democratic legislator. The difference between the voting patterns of the late nineteenth century and those that are found today is that the constituent bases of the two parties were ethnic and religious and the issues that divided Democrats from Republicans were social. Today the constituent bases and the partisan issues involve economic interests and questions, as a consequence of the New Deal legacy. In fact, the social issues that united the parties of the 1880s and 1980s are somewhat comparable to the social issues that divide the parties today.

Although the study of voting alignments and the role of party may be the most pertinent aspects of this study for political scientists, there are also valuable accounts of the issues that captured the attention of legislatures, the background and recruitment of legislators, and the operation of the "amateur legislature," most of whose members were freshmen. Campbell makes a convincing case that state legislatures deserve more attention for historians seeking to understand political developments in the late nineteenth century. Such studies also offer valuable perspective to those trying to understand state legislatures today.

MALCOLM E. JEWELL

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The Economic Crisis and American Society. By Manuel Castells. (Princeton, N.J.: Princeton University Press, 1980. Pp. xiv + 285. $20.00, cloth; $7.50, paper.)

In this exceedingly ambitious, stimulating and frustrating work, Manuel Castells attempts to begin to explain the economic crisis that has shaken advanced capitalism for roughly a decade by trying to explain the crisis for the core case of the United States. He characterizes the crisis not merely as one of stubborn stagflation, but, more fundamentally, as a structural crisis in which reproduction of the political economic system requires basic structural reform or transformation, "as multidimensional, political and ideological, as well as economic" (p. 14). He seeks not only to explain the crisis' origins and trajectory, but also to identify the major coalitional and strategic options that might effect its resolution.

Most works eschew highly complex *explanada,* or pursue them by means of rather blatant simplifications. They engage in narrative, analytic or theoretical prolegomena to actual explanation, or they argue the power of simple theses. Castells boldly pursues the complex explanatory ideals of a broadly "structuralist" neo-Marxism. As regards the economic crisis, this entails two major tasks: (1) articulation of an *abstract,* multidimensional, dynamic theory of crises in advanced capitalism; and (2) *specification* of the abstract theory to the concrete instance of contemporary U.S. capitalism—in a manner that elaborates the theory in light of the idiosyncracies of the case, before adjoining it to the facts of the case in the task of explanation. It also entails a primary reliance upon such key Marxist concepts as exploitation, class struggle and uneven development.

Castells' abstract theory of crises emphasizes factors that tend to dampen or buoy profit rates. Of course, for a theory of crises, dampening factors are particularly important. Castells emphasizes the dampening effects of increases in the "organic composition of capital" (i.e., the value of physical capital relative to that of labor inputs) —despite considerable equivocation. His logical and empirical grounds for this theoretical choice do not overcome the serious objections to an "organic composition" theory of declining profits that had been raised by Joan Robinson, Paul Sweezy, N. Oshikio and others before Castells' writing and have been compounded by Thomas E. Weisskopf, John Roemer and others since. As a result, Castells' major abstract thrust emerges rather lame. Abbreviated discussions of other more promising profit-decreasing factors do little to overcome this misplaced, or, at best, misargued, emphasis. Neither do excellent delineations of such profit-buoying "counter-tendencies" as sundry state-facilitated expansions of demand, since Castells must explain *declining* profits.

The *specification* of the abstract theory to the U.S. is enhanced by the extent of accompanying theoretical elaborations. Perhaps the most interesting elaborations are those arguing that state and corporate effects to stimulate demand have tended in the "long run" (of Keynes' perhaps nervous joking) to lower productivity, stimulate inflation, weaken comparative advantage and otherwise reduce profits. Also noteworthy are Castells' discussions of the class character of corporate authority, the dependence of "post-industrial" tertiary expansions upon industrially mandated state expansions, the fundamental conditioning of state economic interventions by heterogeneous industrial and market structures, and many other antidotes to the conventional wisdom. Unfortunately, these riches may be too tarnished by a mode of presentation which abounds in logical ellipses, facile uses of evidence, and easy convictions to appeal to those not persuaded of the force of Castells' approach and formulations from the start. These riches' failure to come together in the intricate and compelling final design anticipated by Castells' introduction will leave as many readers (of all persuasions) feeling as disoriented as enlightened.

Nevertheless, instructors in courses on political economy, neo-Marxist theory and the ongoing economic crisis will want to consider using this book as a major text, despite its unevenness and shortcomings. Scholars pursuing its *explanada* will want to mine this book for insights. Those erudite and enterprising enough to approach explanation of the economic crisis without assuming away its complexity have much to learn from coming to terms with this major precedent in their course.

ALEXANDER HICKS

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Dean Rusk. By Warren I. Cohen. (Totowa, N.J.: Cooper Square Publishers, 1980. Pp. xii + 375. $22.50.)

Twelve years after the Johnson administration we have the first serious study of Dean Rusk's impact upon our foreign policy in the latest (Vol. 19) of the lengthy series upon American secretaries of state and their diplomacy. So ramified have influences on policy become that the secretary of state frequently can seem one of peer officials and consultants projecting views upon the president. Personality then can prove more important than formal position. To describe the secretary's role may involve fitting random pieces from a piled jigsaw puzzle. A further complication may be source availability. Secretary Kissinger's precedent reserved the substantive files of his incumbency for a durable period for his own use.

To examine Secretary Rusk's place under Presidents Kennedy and Johnson, Warren Cohen lightens his load. In nearly 100 pages he explores Dean Rusk's earlier significant career, his mindset through events of his time, his disciplined conceptions of service, and his style of working with associates and superiors. This proves an excellent entry into the portrayal of his eight years as secretary. The perspective is of a good soldier who gives recommendations and who, even if he loses, loyally supports the decisions.

Military staff service and Department of State positions under Marshall and Acheson shaped his practices. Marshall, more his model, sought major sides to a question, heard the views of all contributors, held his cards close, and then privately, as he deemed his duty, expressed his opinions to the president. This explains accounts of presidential meetings with advisers which frequently depict Rusk as probing those present rather than openly expressing his opinions.

Rusk saw the tragedy of World War II as stemming from failure to face down ever-bolder aggression. Confronted by the faceted menace of communism, the United States had to show friend and foe that it would stand by even the vaguest commitments and steadfastly oppose this aggression in whatever form as in the national interest. It became a trial of wills.

President Kennedy and Rusk had an initial shakedown cruise. There was the contrast of impetuosity and action with thoughtful realization of limits. Accounts of people like Robert Kennedy include jabs at Ruskian caution, though Cohen finds that the president appeared satisfied with their relationship. But he did not press Rusk to stay when the latter stated his intention to leave after the 1964 election. With Johnson there were such excellent relations that at his term's close he wrote that Rusk "has served me most intelligently, faithfully and nobly" (p. 316). In fact, Rusk was the last of the 1960 top policy advisers.

A working concept for Rusk was his "little threads," meaning that by varied agreements all countries, including the communists, should acquire interdependencies to maintain peace. As a strong advocate of the United Nations, Rusk tried to use it wherever feasible. Differentiating him from his Europe-oriented deputy, Ball, was a reach of strong interest as an American liberal toward the Third World, Eastern Asia, and their problems.

Much has been written of the dangers of nuclear war from the Cuban missile crisis. Little of the timely crucial information from America's reliable Kremlin spy Penkovsky, radically reducing that prospect by reporting the Soviets had a crushing nuclear inferiority. Accordingly, Cohen asserts that the crisis was perhaps avoidable, and dryly declares that "the only clear and imminent danger . . . was [to] the political future of the Kennedy Administration" (p. 160).

Vietnam, the overwhelming issue, is traced from early American anti-communist support through the tragedy of full involvement. Amid the hawks around Kennedy, Cohen shows Rusk as most reluctant to resort to the military weapon and invariably seeking to limit it. He rightly believed South Vietnam needed basic civil institutions, or any military shoring would be fruitless. A deep service loyalty carried him in the Kennedy years. Johnson then used his tremendous persuasive powers, Rusk signed on for the duration, and Vietnam became a test of his principles. While others left the ship, he sought ways within his grasp to aid American interests and his president. He had given his word—despite some courses of which he was not aware and men's purposes not always his. A political liberal, Cohen strongly faults him for this, while conceding that he was a better man than his presidents. Here is the insoluble dilemma: moral man acting in a morally deficient political world.

Besides documents, Cohen has happily relied heavily upon interviews with well-placed people, including two with Dean Rusk. Surprisingly, a culminating chapter depicting how Johnson changed his Vietnam course has no reference to a Rusk interview or to his comments upon this decisive period, while apparently citing him through others. We have as yet no documented references to the all-important Tuesday luncheon meetings of Johnson with his core advisers. Only now Secretary Dulles is being reevaluated from recently declassified Eisenhower library material. Rusk has written no memoirs. Thus, while the present important, varied sourcebook provides valuable personality and factual insights, we must still await the definitive work.

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National Health Insurance: Conflicting Goals and Policy Choices. Edited by Judith Feder, John Holahan, and Theodore Marmor. (Washington, D.C.: Urban Institute, 1980. Pp. xxi + 721. $25.00, cloth; $12.50, paper.)

For many of us in public health, the question of national health insurance has become such a continuing item of unfinished business that the thought or prospect of still another volume devoted to this question seemed not only unnecessary, but even redundant. Almost everyone associated with the field has provided analyses of not only why there is no national health insurance program in place, but also why the current proposals are inadequate, however needs are defined or perceived. Thus it is refreshing to examine a book offering a different perspective and avoiding many of the pitfalls to which other attempts to illuminate some of the more vexing issues in this arena have fallen prey.

My generally positive feelings about this volume derive primarily from its focus, which parallels a strong bias of mine. It has long been my judgment that the critical dimensions in the success of any program—be it health insurance or highway construction—are those relating to management and administration. Good policy arises as much from good choices as from how that policy is implemented and how it ultimately affects the citizenry. In their introduction, the editors clearly suggest that one, if not the principal, concern of this work are those aspects of policy choices which deal with how any given policy concerning national health insurance will, in practice, meet the goals set forth. To a large degree, they have succeeded in keeping the several essays in this volume directed towards that central issue.

Second, the essays avoid many of the ideological arguments which so frequently clutter debates about national health insurance. While the salience of ideology is clearly recognized, most of the essays give primary emphasis to what can be accomplished under various schemes, given the existing governmental, corporate and professional systems currently involved in providing medical care. It is contended, and correctly so, that many of the issues of implementation are common to all of the present proposals for a national health insurance program, and that how these questions of execution are met will have a greater impact on whether care is actually received by those in need, than the degree to which a plan may conform to one or another of the political or professional prejudices which abound in this area.

This volume differs from others in arguing that existing state and local governments should be involved in implementing a national health insurance program. This posture is in stark contrast to most discussions in this area which tend to be heavily centralist in their tone. The opening chapter, "Administrative Choices," by Feder and Holahan offers a first-rate analysis of the various institutional and organizational arrangements for providing medical care and an assessment of these for any prospective national health insurance endeavor. By avoiding the assumption which has affected so many policy analysts that wisdom and goodness primarily resides in Washington, the authors point to clear examples of where and how states and localities can participate effectively in the management and financing of a national health insurance program.

For political scientists and others familiar with the substantive questions involved in national health insurance, the book provides little new information. What it does do is to take such matters as the breadth and level of eligibility, scope of benefits, payment and regulation of providers, assurance of quality, utilization and the experiences we have had with the various means for dealing with these issues as a basis for analyzing several different national health insurance proposals. This tack places such questions in a different perspective and leads one to look at what specific policy choices may mean in the context of given resources, organizational arrangements, professional needs and related considerations.

The concluding section summarizes the several essays and, at the very end, projects some policy recommendations. These, at least in my judgment, deserve more direct attention than they have been given. An example is the statement that broader access to health care and cost containment can only coexist if government asserts its responsibility for allocating resources. This, in turn, requires formal mechanisms for deciding what we as a society want to spend on medical care. Such a strategy contrasts directly with present policy in which levels of expenditure are largely determined by providers who negotiate acceptable levels of reimbursement for specific items of cost. A further exploration of the implications of this change in policy which reverses roles of the public and professional sector would be most illuminating, not just for the discrete elements of a national health insurance program, but in an overall sense.

This volume is further enhanced by a good bibliography, and an appendix in which the features of eight of the most recent proposals for national health insurance are outlined in tabular form. As both an introduction to this complex field and as an antidote to some of the rhetoric which surrounds the debate in this area, *National Health Insurance: Conflicting Goals and Policy Choices* is worth having on one's shelf.

JOHN H. ROMANI

University of Michigan

The New Senate: Liberal Influence on a Conservative Institution, 1959-1972. By Michael Foley. (New Haven, Conn.: Yale University Press, 1980. Pp. x + 342. $17.95.)

In *The New Senate: Liberal Influence on a Conservative Institution, 1959-1972,* Michael Foley deals with an important topic and touches on a number of important themes. Foley takes as a point of departure the descriptions and analyses of the Senate of the 1950s by William S. White (*Citadel: The Story of the U.S. Senate,* 1956) and Donald R. Matthews (*U.S. Senators and Their World,* 1960): a conservative institution with structures and norms which supported the status quo and where influential and effective senators were those who conformed most closely to the norms; liberals tended toward nonconformity and frustration, and were generally unable to produce liberal policy outputs. Foley focuses on the activities, behavior, and policy successes of Senate liberals in order to "examine the questions of change and to assess how much, if any, of White and Matthews' classic model remains intact after the developments of the 1960s" (p. 14) and, in the final chapter, to "attempt to assess not only the degree to which the liberals changed the Senate, but the extent to which the Senate changed them" (p. 15).

Foley's conclusions are not surprising—an identifiable group of liberals was active in the Senate during the period studied, and changes in norms, structure, and legislative output occurred. More specifically, (1) the number of liberals in the Senate varied, reaching a peak in the first Johnson Congress (1965-1966); (2) liberals are not all alike: the size of the liberal group varies from issue to issue (civil rights had the most support and civil liberties and congressional reform the least); (3) liberals became more effective as their numbers increased and they began to occupy positions of power; and (4) the external political environment, especially presidential positions and citizens' attitudes on issues, are important factors in group effectiveness.

The chapter which deals with American liberalism and Senate liberals is the strongest and most successful of the book. Foley recognizes the problems of conceptualization and specificity in the designation "liberal." His solution is ingenious and sophisticated. Using the public record—e.g., newspapers, journals, Senate documents—he identifies concerns which were perceived as dividing the polity and the Senate along liberal-conservative lines. Then, using individual roll-call votes and Guttman scaling, he identifies liberal-conservative issues in the Senate and liberal senators. For each Congress he constructs three to seven scales (civil rights, education, foreign aid, etc.); a senator's position on a scale results in a score. Foley derives a composite index for each senator based on the proportion of "liberal" scores. He then classifies each senator on a continuum from liberal to conservative. (These data are displayed in some detail in the appendices.) Foley's decision rules are clearly stated and the data carefully interpreted. As he notes, his techniques of measurement are not without problem: for example, all liberal issues are treated as of equal importance. Nevertheless, the approach is useful. The results of the operational definition fit the real world (it is not just a skillful technique), and the measures take into account the multidimensionality of liberalism, overcoming a major problem of impressionistic data and single-index measures.

Chapters on Senate folkways, the liberals as a group, and the liberals as a legislative force are less successful. Foley asks the right questions: have norms and folkways changed; are liberals a group, with ongoing communications networks, formal structure, and cohesiveness; and how have liberals affected policy outcomes? The conclusions, however, are based primarily on illustrative evidence from case studies and interviews, and it is difficult for the reader to judge the extent of change or the validity of the findings. The analysis would be richer and more persuasive if Foley used his extensive interview data more systematically.

Throughout the study Foley deals with a number of important themes: the relationship between the political system and a political institution; how organizations, and in particular legislative institutions, change; the interaction between an institution and its members. There are also interesting questions about ideological groupings in legislatures: for example, whether informal and changing coalitions are truly groups, and the effect of formal structure on policy outputs. The discussion, drawing on organization theory, is often perceptive although the treatment is uneven with little systematic analysis.

The strengths of this study lie in the questions raised, in the definition and measurement of liberalism, and in going beyond roll-call analysis to explore the operation and effect of ideological groups in Congress. But readers will also find the study frustrating because the questions raised are not fully explored and there is too little systematic evidence to support many of the conclusions.

SUSAN WEBB HAMMOND

American University

Jimmy Carter: In Search of the Great White House. By Betty Glad. (New York: W.W. Norton, 1980. Pp. 546. $19.95.)

The official excuses for Jimmy Carter's failure to win reelection have already been articulated by the president and his White House aides. No president can govern, they say, in the face of fragmented political parties, single-issue interest groups, a democratized Congress, and unprecedented economic problems. The president, they add, did not worry about "politics" but faced the hard questions about the Panama Canal, the energy crisis and human rights around the world. Anyone who does this, they argue, will wear out his welcome.

This defense leaves no place for skill by a president at winning political support for his policies. It is fashionable among political scientists today to argue that the importance of the skill factor has been overrated by presidential scholars. The Carter presidency is an interesting test case of the thesis.

The argument will not work. Carter's failure was that he did not address himself to the problems which were worrying most people. Few people care about government reorganization or even the Panama Canal. But inflation and the power of the U.S. in the world are of concern to people.

Carter lacked any sense of political strategy and thereby the majority of citizens came to believe that he was not in control of the events which most concerned them.

A biography of Jimmy Carter is therefore more than the story of a man. The study of his presidency may permit us to assess the importance of skill in relation to the many constraints on leadership in American politics today.

Betty Glad does not address these larger questions in her biography of Carter. She wishes to understand him as a person. She begins her search with two puzzles. First, Carter's career displayed great drive for political office but no evident passion for the achievement of principles like justice. Second, he is good at winning but poor at governing.

She documents with exasperating thoroughness the early life of a young man who had no outstanding achievements, yet believed himself destined for greatness. Although she does not make the point, the contrast with Harry Truman is apparent. Truman attained more than he sought and his humility was genuine. Glad focuses on Carter's habit of pretending to be more than he is in the public persona which he has created. For example, he later falsely described himself as a finalist for the Rhodes Scholarship when he had been eliminated early in the competition. There are many little stories like this and they add up to a man who wishes to be more than he is.

In his election to the Georgia Senate, Carter manifested the political style which was to be his trademark: intensity, ability to create a good press, the portrayal of himself as an innocent, and quiet manipulation of political organization. Glad points out but does not explain the combination of moralism and willingness to go for the jugular of his opponent.

As a gubernatorial candidate, he was a centrist who avoided labels and sent mixed messages. He ran as an outsider against Atlanta and its "establishment" but his populism was "stylistic" rather than substantive. His program was one of "good government" with administrative reorganization as the top priority.

Glad foresees Carter's style as president in his governorship. He achieved most of his legislative programs through a combination of frontal assault and private trimming, but he was never comfortable in his relations with legislators and resented opposition as somehow immoral. She concludes that he was a good but not extraordinary governor, who might have had difficulty being reelected.

Carter reenacted the entire Georgia story on the national stage. Glad originally set out to tell the story of the 1976 campaign, but since Carter won, she gives us all the reenactment except for the denouement in 1980. However, there is more material on the campaign than anyone would wish to know. The primary systems and heightened role of the media were perfect for nan outsider candidate who made himself the issue. Carter understood that Americans yearned for a restoration of "good authority." He asked them to trust him and projected himself as a man capable of greatness.

Her chief criticism of Carter as president is that he brought no economic or social philosophy to government. We again see the emphasis on procedures—reorganization, zero-base budgeting, civil service reform—and efforts at comprehensive solutions which floundered for lack of political preparation. But we also see successes like the Camp David agreement which stemmed from the same intensive homework which blocked any larger vision. She ends the narrative with Carter's skillful use of the hostage issue to protect himself from criticism in the 1980 primaries. We see the campaigner who cannot govern.

Her final picture of Carter is a long way from James David Barber's "active-positive." She sees him as a man who is so in control of himself that he lacks creativity, who lacks a sense of the tragedy in history and believes that good intentions and practical reason can solve all problems. Inevitably, he promises more than he can achieve.

The author invokes Karen Horney's model of the expansionist, narcissistic personality to explain Carter. The expansionist seeks to attain an idealized self who has mastery over all problems. The drive has a compulsive nature and reflects a deep psychological need for victory and recognition. It is winning that matters, not what we do with victory. Thus, the skills which enabled Carter to win elections served him poorly in office because one does not govern by manipulating images of the self.

It now appears that Jimmy Carter was defeated in 1980 because many voters did not believe that he was in command of events or of the presidency. Betty Glad helps us understand the psychological roots of this failure. Carter was never fully in touch with the issues that most concerned people. He never gave sustained attention to economic problems or relations with the USSR or even relations with the Congress. His energies were devoted to many peripheral issues in the search for the appearance of mastery.

Any Democratic president might have failed in these years in the face of the exhausted agenda and divided coalition of liberalism. But Glad would not regard that as an excuse for Carter. Her criticism is devastating, for she charges that the man is incapable of dealing with the world in realistic terms. She does not provide the final or definitive word on this question, but future biographers will have to take the argument very seriously.

ERWIN C. HARGROVE

Vanderbilt University

A Policy Approach to Political Representation: Lessons from the Four Corners States. By Helen M. Ingram, Nancy K. Laney, and John R. McCain. (Baltimore: Johns Hopkins University Press, 1980. Pp. xviii + 270. $22.00.)

Representation, with good reason, has been a central concept and a continuous source of dispute wherever democracy has been tried. And that has given rise to a long debate among political theorists about the problems of fair representation, of even the question whether the many can be fairly represented by the chosen few. More recently the subject has been a focal point of empirical research, producing an enormous body of literature devoted to comprehending the opinions of the represented and comparing those with the ideas and acts of the representative. That literature has emphasized the public's lack of sophistication in political matters; indeed in some instances the public appeared downright stupid and self-contradictory. V. O. Key, however, reviewed the findings of these analysts and devoted a book (his final one) to the "perverse and unorthodox argument . . . that voters are not fools. . . ." Helen Ingram, Nancy Laney, and John McCain take up a position beside the master, finding considerable agreement about policy preferences between legislators and voters—at least in their corner of the world.

In many ways this work is a tidy little gem; its research techniques are imaginative but sound; its analysis is persuasive. And if the writing is occasionally cluttered with jargon, Ingram, Laney and McCain sin far less than most of their contemporaries doing comparable work. Essentially, they have sought to find links between state legislators' actions and the opinions of their constituents on matters of significant public policies. The scene is the "Four Corner States," so called because the borders of Arizona, New Mexico, Colorado and Utah meet at a single point, the only place in the country where that happens, but it is not this geographic coincidence that is important; rather they are states with rapidly growing populations and a host of environmental and other problematic questions—like the question of water supply in that often-arid countryside.

The raw material of the work consists of interviews with legislators and questionnaires sent to voters. The writers sought to interview all of the senators from the four states and managed to converse with 9 out of 10 of them. The questionnaire went to 50 voters in each senatorial district and nearly three-quarters of the recipients responded (some after persistent prodding). They then proceeded to compare the opinions of both voters and state senators across a fairly wide range of policy questions. In a number of graphs showing the distribution of voters' and legislators' positions on 10 environmental questions, there is a surprising consistency of viewpoints between voters and representatives. Of course there are also differences between the two groups; legislators were more inclined to be guided by party positions and ideology than were voters. Moreover, there were differences among the four states, and one might expect that states with other traditions—particularly states with strong party influence and tight discipline—might differ even more were these methods to be applied there.

Nevertheless, one should give a fair hearing to good omens regarding the operations of democracy these days. It is true that the authors do not deal with the divisive conflicts that now agitate older, more highly urbanized states, but what they have given us merits attention and repetition elsewhere.

DUANE LOCKARD

Princeton University

Public Representation in Environmental Policymaking: The Case of Water Quality Management. By Sheldon Kamieniecki. (Boulder, Colo.: Westview Press, 1980. Pp. xi + 131. $20.00.)

It is encouraging that there are political scientists who still see the environment as a significant issue worthy of study. Sheldon Kamieniecki examines comparisons of leaders' and citizens' beliefs and leaders' perceptions of public beliefs on water quality issues. It includes a concise history of federal water pollution legislation, noting the effects of lobbying in Congress which has resulted in past postponement of federal deadlines. Section 208, of the 1972 Federal Water Pollution Control Act Amendments requires citizen participation in the development and implementation of regional water quality plans to meet national water quality standards. Kamieniecki states his primary concern as the need for greater citizen participation.

The major survey from which the data were drawn was conducted by the Environmental Studies Center of the State University of New York at Buffalo and consisted of questions asked of leaders and citizens of the Niagara Frontier region. Some of the survey data results were not surprising, namely, that most people are reluctant to take an active role in planning, and the less educated are more willing than the general public to leave planning to the specialists.

A difficulty with some of the questions was whether the questions asked really prove the hypothesis posed. For example, Hypothesis 2 stated that blacks will be more supportive of policies that improve their surrounding environment than the general population. According to the author, the data did not support the hypothesis. But the specific questions asked of respondents concerned planning, relinguishing local power to a regional body and a government's right to property—questions which do not necessarily satisfy the meaning of the hypothesis. Blacks may yet support more specific or qualitative environmental goals such as clean drinking water, parks or clean air while not understanding how the governmental mechanisms described in the questions would improve the environment.

An interesting finding was that the residents of the Niagara Frontier "were willing to trade-off their freedom to do as they wish in favor of planning what was best for their community" (p. 52). Kamieniecki argued that restrictions on personal freedom might be necessary to improve water quality. But the respondents were not asked if they would give up personal freedom for some specific end. The use of a question with some boundaries, e.g., mentioning what the goal was or how giving up personal freedom to obtain that goal, might yield a more meaningful result.

On the question of how well leaders perceive the public's views on environmental issues, it was found that "208" planners and environmentalists hold beliefs similar to those of the broad public but that they fail to predict the beliefs of the public. Even if leader perceptions were more accurate, many of the questions asked in this survey were of a general nature and could not serve as a guide for leaders.

Nevertheless, the data presented by Kamieniecki raise some interesting questions about the disparity of leader's perceptions of citizen opinions and public opinion itself. The author offers some explanation in chapter 5, i.e., leaders are overly cynical and misjudge public opinions because of their eagerness for results and their complete involvement in their work. It might be worth asking whether this is characteristic of environmental policy makers or other leaders as well.

The final chapter deals more directly with the nature of citizen participation. The data reveal that the actual beliefs of citizens and leaders are similar on many issues. It is the perceptual accuracy of leaders which is problematic. The author suggests that because of the disparity between public opinion and leader's perception of public opinion, citizen input is inadequate. Further research might focus on whether the coincidence of beliefs between leaders and citizens is a stable relationship or if it is susceptible to change as new issues emerge.

The author concludes by saying that officials should be given more accurate information on public opinion (p. 88). This final chapter discusses mechanisms (public hearings, workshops and ad hoc committees) which could be employed toward that end. However, for the public to register meaningful public opinion, they also must have timely and accurate information.

This book, published as a Westview Replica Edition, is representative of a trend to accommodate the increasing number of publishable, specialized and scholarly works on an economical scale through the use of camera-ready copy. However, the lack of professional editing (correction of spelling, typographical errors, incomplete sentences and awkward grammar) can detract from the appearance and impact of a book. Greater use of headings and transitions between sections in this instance would have added to the clarity of thought and would have alerted the reader to subtle changes in the discussion. Nevertheless, Kamieniecki's effort should be commended for its attempt to link political participation with environmental issues in a thoughtful and concise manner, and I hope that other political scientists will continue to address issues concerning political participation and the democratic process in an environmental context.

REGINA S. AXELROD

Adelphi University

The Press as Guardian of the First Amendment. By John Lofton. (Columbia: University of South Carolina Press, 1980. Pp. xiv + 358. $14.95.)

The press has not served as an effective guardian for freedom of expression, according to this first historical survey of newspaper reaction to free press and speech issues. John Lofton, an editorial writer for the *St. Louis Post Dispatch,* a historian and attorney, contends that the general circulation press has usually not supported those with unpopular views. The newspapers ordinarily champion free expression only when their own vested economic interests are involved.

In this critical analysis of the press, pre-World War I incidents receive extensive coverage. Relying on James Morton Smith's *Freedom Fetters: The Alien and Sedition Law and American Civil Liberties* (Cornell, 1956), Lofton states that the Federalist press called for prosecution of Republican editors under the 1798 Sedition Law. Making extensive use of Russell B. Nye's *Fettered*

Freedom (Illinois, 1972), Lofton contends that the southern newspaper editors "applauded restrictions" (p. 101) on the abolitionist press. Destruction of an abolitionist press in Cincinnati was supported, with one paper calling it "the most systematic, orderly and well behaved mob we have ever witnessed" (p. 90). In the post Civil War era big newspapers pursued the attitudes of big business and did not defend the radical labor press.

More restrictions were imposed on freedom of expression during World War I and its aftermath. Yet there was almost total abdication of the general circulation press of any responsibility to uphold freedom for dissent. Lofton repeatedly criticizes the *New York Times* for its lack of concern for free expression and its support of anti-civil libertarian court decisions, legislation, and executive actions such as the Palmer Raids.

The press has been reluctant to support freedom of expression against government claims of national security as during the McCarthy era. In the Pentagon Papers case of 1971, Lofton contends the *New York Times* made "narrow and timid arguments" (p. 259) and not a lofty appeal to the First Amendment rights. Even though the resulting limited victory against prior restraint was widely supported in the press, the media did not mount a campaign to end excessive government classification and secrecy. C.I.A. censorship received limited editorial attention even from the papers enjoined in the Pentagon Papers controversy. The right of the *Progressive* magazine to publish a hydrogen bomb story in 1979 received ambivalent media support.

Lofton recommends that the newspapers should be active and articulate champions of the rights of free expression. Citing problems raised by Herbert Marcuse, he urges papers to allow substantial access on a regular basis for those who do not own a press and who have conflicting or unpopular views.

Several methodological problems can be identified. Less than half of the book covers events since World War I—the time when Supreme Court decision making on First Amendment rights began. Detailed accounts are given to problems prior to World War I while some recent issues are inadequately discussed. Furthermore, parts of the book are concerned primarily with general first amendment history and not press reaction. Lofton makes extensive use of the primary source of newspaper editorials, but he fails to explain why particular papers were selected, i.e., circulation, reputation, section of country or availability. Tables and charts on his content analysis research would be useful in showing the extent and dimension of media support for different issues.

Lofton's heavy reliance on newspaper editorials as a primary source causes one to question the comprehensiveness of his account. While newspaper editorials are one significant factor in determining the response of newspapers to First Amendment issues, additional research approaches could be used. A study of interest-group tactics and strategies in *amicus* briefs, lobbying, funding, and education could show other patterns of reaction.

Lofton's work is an informative historical account on the failure of newspapers to champion free press and speech editorially. His general arguments are persuasive; however, a definitive and systematic analysis of press reaction to free expression issues still needs to be pursued.

BYRON G. LANDER

Kent State University

The State and Human Services: Organizational Change in a Political Context. By Laurence E. Lynn, Jr., with the assistance of Timothy C. Mack. (Cambridge, Mass.: MIT Press, 1980. Pp. xii + 217. $15.00.)

This book is in a series of monographs that combine substantive public policy analysis with "insights into the workings of American political institutions" (p. vii). Lynn analyzes processes of organizational change in human service programs and agencies in six states: Arizona, Florida, Georgia, Minnesota, Pennsylvania, and Washington. He focuses on decisions made by the originators of change proposals to ascertain what they were seeking, the processes of debate and deliberation, and the results. The governors of Georgia and Washington initiated changes and had to combat legislative and bureaucratic inertia. Arizona and Florida legislators were the advocates of comprehensive reorganizations, and they had to fight indifferent or reluctant governors. Minnesota officials adopted a gradual, modest approach to reorganization, while the advocates for change in Pennsylvania encountered "numbing inertia" from the governor and legislative leaders.

Guided by the assumption that most public policies are products of a continuous political debate over the proper scope of authority for the states and the national government, Lynn prefaces his cases with a chapter on "The Changing Shape of Federalism." In the evolution of human service programs and organizations, a fundamental tension has developed between the advocates for the needy (reformers and service professionals) and public officials. This schism, Lynn says, was born out of each group's definition of need. Professionals tend to be absolutists and define need in philanthropic terms: "A person who can be helped should be helped." But officials take a more pragmatic view "and weigh the claims of advocates for service expansion against the availability of resources and the preferences of the larger community" (p. 14).

By the 1970s, several important strands of organizational change meshed to shape state reorganizational debates: greater executive control, growing resistance to federal control, and a call for more service integration. Lynn shows how these issues were chewed up and digested in the states. He also shows how individual state politics renders different substantive results. In Florida, Georgia, and Arizona, for example, changes were introduced from the "top down," and emphasized "service integration . . . as an economy measure or as a means of achieving greater control over categorical program interests" (p. 150). In Minnesota, on the other hand, because of the strong role of county government, change was designed from the "bottom up" to emphasize improvements in service delivery. Lynn's prescription for success is the Minnesota approach. "Though significant changes in status quo have occurred in Florida, Georgia,, and even in Washington and Arizona, there is simply no firm evidence that economies, cost containment, or more effective services have been the result. Gradual change built on consensus may be, in the end, the only kind of change that can bring about genuine and verifiable improvements in service delivery" (p. 151).

Case studies can cause analytical headaches. After disassembling the cases, analysts must then deal with the thorny problem of what they mean individually and collectively. As they proceed through this exercise, they may feel as frustrated as all the King's horses and all the King's men when they tried to put the pieces back together again. But, where they fail, Lynn succeeds. He not only unravels the nuances of individual cases, he deals with the Humpty Dumpty problem by formulating "general propositions about both the processes and the character of organizational changes in the states. . ." (p. 173). While a short review cannot do justice to Lynn's synthesis, two conclusions merit mention. First, regardless of the initiator, Lynn concludes, "The pace, character, and success of organizational change are colored by the governor's performance" (p. 174). Second, "elected state officials have on the whole a greater sense of responsibility toward economy and efficiency in governmental operations than they do toward the effectiveness of coverage in human services" (p. 178).

Lynn has simply written a successful book. With the aid of a Mack, he trucks us across six states pointing out individual characteristics or reorganizational politics. Because he is guided by a well-thought out plan of analysis, at journey's end he is able to provide us with an overview of where we've been and the significance of the trip for future excursions into the morass of state politics and organizational changes. What more could we ask?

ROBERT D. THOMAS

University of Houston

Unelected Representatives: Congressional Staff and the Future of Representative Government. By Michael J. Malbin. (New York: Basic Books, 1980. Pp. viii + 279. $15.95.)

Two minor liberal myths about staffing cohabitate uncomfortably. One is that the heavy workload of modern politicans surely can be made manageable if they have adequate expert staffs; the other is that there is an inevitable Parkinson's Law in staff growth which ultimately leads to staff control of elected politicians. Three decades ago, the clamor was for extraordinary acceleration in the growth of congressional staffs. Today, there is a kind of thermidorean worry that things have gone too far; that the staff has come to be on top rather than on tap. Recently, congressional staffs were said to be "the invisible force in American lawmaking" (Harrison W. Fox, Jr., and Susan Webb Hammond, *Congressional Staffs* [Free Press, 1977]). Now, they are "unelected representatives." The implication is, of course, that Congress is being run, and public policies legislated, not by our elected representatives, but by the invisible, behind-the-scenes manipulators serving on members' and committee staffs who are not accountable to the voters of the country. No close observer of congressional life would want to deny that staffs are influential, but that is a far cry from the claim that staffs control Congress.

In fact, no such claim is substantiated in this book. Michael Malbin is properly concerned about the appropriate role of congressional staffs. His interests in the extent to which these staffs have alleviated the heavy burden of congressional work, and effected changes in the capacity of members of Congress to serve as representatives, lead him to focus upon staff activities in various legislative settings. His book is an exploration of the staff role in these settings, and so it reads as a rather nicely integrated series of case studies. The staff director of the Senate Select Committee on Small Business shapes the committee's working agenda; the staff of the Senate Government Operations Committee's Subcommittee on Intergovernmental Relations "shepherds" the Sunset Bill through the Senate; House and Senate staff members negotiate intercameral differences over the

GI Bill (the "case of the phantom conference"); the staff of the House Energy and Power Subcommittee engages in oversight activity; the nonpartisan staff specialists of the House Budget Committee make policy recommendations to members based on their economic projections; staffs of the Congressional Budget Office, the General Accounting Office, and the Congressional Research Service provide "policy analysis" on the issue of natural gas deregulation.

Malbin's principal purpose in telling these stories is to make the argument that congressional staffing has developed in such a way as to encumber the deliberative function of Congress. First, he argues that the large congressional staffs, and particularly the so-called "chairmen's staffs" of committees, attenuate the flow of reliable information from staff to committee leaders, and from committee leaders to rank-and-file members of Congress. Moreover, as gatekeepers for the schedule of members' time, the staff screens information and contact in a self-serving manner. Here, amid a melee of anxieties real and imagined, is the crux of Malbin's argument:

> Since the staff people are not themselves elected, and since their future careers in the Washington community often depend on their gaining reputations as innovators, it is to their interest to spend time listening to people whose ideas may help them put something new on the oversight or legislative agenda, even if those people have no political constituencies of their own (pp. 248-49).

This kind of staff screening tends, Malbin believes, to be quintessentially technocratic and ideological. To the extent the information flow is interdicted in this way, the congressional process is depoliticized.

Second, he contends that the staffs, and especially the "entrepreneurial" ones, exacerbate, rather than assuage, the problem of congressional workload. Thus, "instead of freeing the members to concentrate, the staffs contribute to the frenetic pace of congressional life that pulls members in different directions, reduces the time available for joint deliberation, and makes concentration all but impossible" (p. 243). Third, the growing tendency for staffs to negotiate legislative issues, instead of members negotiating with one another directly, leads to mere "deals" between self-interested parties, much as lawyers make settlements for their clients out of court, rather than the kind of give-and-take among elected representatives which is aimed at making decisions for the common good.

Malbin offers little in the way of proposals to improve the Hobbesian condition of the modern Congress. He momentarily toys with the idea that each committee should have both a non-partisan professional staff and a personalized, entrepreneurial, political staff, but despairs that this would solve anything. He considers the possibility of reducing the magnitude of the congressional agenda, but concludes that the complex agenda of government as a whole would have to shrink to effect a smaller congressional workload—a prospect he does not foresee. He cannot prescribe a solution for the increasingly technical character of public policy issues, or the tendency for members of Congress to deal with them in technical, rather than in ordinary political, terms.

This piece of investigative reporting presents a challenge to the adequacy of representative government, as representation was understood by James Madison in *Federalist* No. 52. Like the Harvard undergraduate who wrote his honors thesis on the Newfoundland fisheries issue from the point of view of the fish, Malbin has adumbrated the problems of congressional deliberation and representation mainly from the viewpoint of the employees. The result is myopic. A more adequate assessment will require taking the perspectives and activities of members of Congress themselves into account. Or, more broadly, the perspective of the institution. Congress as an institution is more than a happy hunting ground for head-hunting, aggrandizing staff. Accordingly, Congress is more than an arena in which staff influence comes into play, as Malbin occasionally admits. It is also a system of constraints—of loyalty to the chairman, limited advocacy, deference, anonymity, specialization, limited partisanship, leadership, organization, and isolation. In recent years these constraints may have diminished, but they have not evanesced. A more balanced inquiry would want to consider such constraints on staff behavior along with staff capabilities for performance.

SAMUEL C. PATTERSON

University of Iowa

The Emigration Dialectic: Puerto Rico and the USA. By Manuel Maldonado-Denis. (New York: International Publishers, 1980. Pp. 156. $9.50, cloth; $3.25, paper.)

The Emigration Dialectic is the English translation of *Puerto Rico y Estados Unidos: Emigración y Colonialismo* which won Cuba's Casa de las Americas prize for the best essay written in 1976. In this study of Puerto Rican emigration to the United States, Manuel Maldonado-Denis states his assumption clearly; this exodus "is not a fortuitous . . . social phenomenon, but rather something profoundly connected to the capitalist-colonial mode of production" (p. 29). Simply put, when the capitalist center needs a cheap and abundant labor force, the peripheral nations provide a ready pool of workers on which to draw (p. 14). For Maldonado-Denis, the labor flow from Puerto Rico to the United States closely parallels the flow of southern European "guest workers" northward—a flow that has helped to create "economic miracles" as in the Federal Republic of Germany.

We are told early on in the work that the choice of "emigration" rather than "migration" to describe the Puerto Rican case is based on politics rather than semantics. One "migrates" within one country but "emigrates" from one country to another. To Maldonado-Denis, Puerto Rico is not part of the United States, but "a Latin American nation which has been under U.S. colonization since 1898" (p. 16). Therefore, he uses "emigration" to highlight the exodus of hundreds of thousands of islanders to a *foreign* nation.

The main contribution of *The Emigration Dialectic* is that each of its seven chapters raises issues that should stimulate further exploration and analysis. In chapter 2, for example, the author discusses "neo-Malthusianism" on the island as seen in the controversy over the sterilization of large numbers of Puerto Rican women. Sterilization and emigration are analyzed as two processes directly related to the island's economic development program and on both issues, according to the author, the Puerto Rican government has acted irresponsibly.

The high points of the book are chapters 6 and 7 on "cultural assimilation" and "those who return" to Puerto Rico. First, Maldonado-Denis analyzes the process of cultural assimilation both on the island and in the States, and concludes that "the difference in the process . . . in Puerto Rico and in the metropolis is one of degree and not of type" (p. 104). In this chapter, the author's discussion brings to mind some of the more moving passages in the works of Memmi or Fanon on the ravages of colonial culture on the colonized. The chapter on the "New Yorricans" who return to Puerto Rico is another perceptive discussion; "New Yorrican" is a term loosely applied to all Puerto Ricans born on the mainland. The mainland-born Puerto Rican may not only feel alienated in the States but may also feel alienated on his return to Puerto Rico. "The so-called New Yorrican is the target of the hostility of those on the island. . . . [He] becomes a scapegoat, the victim of a national chauvinism" (p. 127).

The Emigration Dialectic is a book written with passion, and so is likely to invite criticism from those hoping for a more balanced expository style. As one who has been critical of Puerto Rico's dependent development and its concomitant emigration process, I found that at times Maldonado-Denis' broadsides against policy makers on the island and the mainland were not sufficiently documented and sometimes his evidence was inconsistent. One example involves unemployment statistics for Puerto Rico. In 1976, the official unemployment rate was 19 percent, but critics of the island's development program often cite an unofficial rate that is about 10 points higher. Throughout the book, Maldonado-Denis shifts between a 30-40 percent rate for the same period and usually neglects to mention that this is an unofficial guess. Another problem involves the book's translation from Spanish to English. At points the translation is not merely clumsy, it renders an idea meaningless.

Despite these reservations, I recommend this as a provocative discussion of Puerto Rican emigration and the dynamics of emigration more generally. By examining the socioeconomic "push" factors in Puerto Rico and the "pull" factors in the States at the height of the Puerto Rican exodus in the late 1940s and 1950s, we can better understand the motives of more recent emigrants from other Caribbean islands, Central, and South America. Finally, students of ethnicity will find this work useful. Maldonado-Denis sheds light on the reasons why Puerto Ricans and other ethnic groups may be resisting the pressure to "melt" in the great American "melting pot."

SHERRIE L. BAVER

City College, City University of New York

Promise and Performance: Choosing and Implementing an Environmental Policy. By Alfred A. Marcus. (Westport, Conn.: Greenwood Press, 1980. Pp. xix + 204. $18.95.)

The Environmental Protection Agency (EPA) and the Clean Air Act Amendments of 1970 (CAA) were more than merely major new bureaucratic and policy mandates. They also represented ideas generated by three important reform movements. Substantively, they reflected the environmentalist demand for a major new government effort in pollution control. Organizationally, they exemplified the long-standing proposal offered by a variety of presidential commissions that specialized regulatory agencies be headed by single administrators who are accountable to the president. And legally, they responded to critics who called for clearer and less ambiguous policy mandates from Congress. It is in these qualities that the EPA and CAA showed great promise; but, as Alfred A. Marcus demonstrates through this interesting case study, it is in these very same areas that environmental policy efforts were doomed to failure.

The lesson Marcus draws from his analysis is at once both simple and profound: if you try to satisfy everyone, you're likely to please almost no one. The EPA's unsuccessful attempts to respond to the contradictory objectives of its two masters (Congress and the White House) as well as the ever-expanding demands of its primary clientele (environmentalists) were further aggravated by the diverse viewpoints which emerged from among the policy, program, and research components of the newly formed agency. Marcus does a credible and insightful job of describing EPA's dilemmas during the 1970s and how that agency tried in vain to contend with them. It is only as he moves away from this major thesis and descriptive task that his study becomes awkward, incomplete, and frustrating to the reader.

Minor themes in the work seem to slip from Marcus' grasp. In a number of places he raises issues and proceeds to abandon them with less than complete analysis or without having joined the specific issues to the case study. Not only is this a source of frustration but it also gives the impression that the author lacked confidence in his own analytic capabilities since he is often diverted from his task in a search for a coherent framework. These problems are most in evidence in the first and final chapters. Rather than focusing on the major theme of the study at the outset, Marcus opens his work with what proves to be an irrelevant comparison of policy evaluation criteria. Only after the first ten pages does he get to meaningful information. Ironically, the final pages of the book suffer from the opposite problem, for it is here that Marcus offers several relevant points on the difference between divided and concentrated authority patterns—points which, if provided much earlier in the text, would have vastly increased the value of the case study. As it is presented, the reader is left "hanging" in this and other parts of the work.

The problems that Marcus faced are not unique among those who study policy implementation and use the case study approach in their analyses. When Pressman, Wildavsky, Bardach and others launched this field of study, it seemed to hold considerable promise as a means for explaining policy successes or failures, and many political scientists picked up the challenge and ran with it. But recent implementation studies indicate that the runners have hit a "wall" and that the work of these analysts has come to a point where the lack of new and insightful concepts and analytic frameworks might render future studies redundant. Marcus' somewhat awkward search for a relevant perspective is a sign that all is not lost.

MEL DUBNICK

University of Kansas

Three Mile Island: Prologue or Epilogue? By Daniel Martin. (Cambridge, Mass.: Ballinger Publishing Company, 1980. Pp. ix + 251. $14.50.)

Like "Pearl Harbor" and "Watergate," "Three Mile Island" has become part of our language. It is the place where millions of people believe that what was supposed to be impossible almost happened: a catastrophic nuclear accident.

Daniel Martin's fine book can be read and enjoyed as a gripping minute-by-minute, even second-by-second, account of what did happen at Three Mile Island between March 28 and April 2, 1979. And, as they say of all good thrillers, "few readers will be able to put it down." I certainly could not, nor could several friends to whom I have passed it along.

But, to Martin's considerable credit, his narrative moves beyond the inherent melodrama of the episode into some fertile territory for reflection about the operation of government. It has been many years since Karl Deutsch first taught us about the importance of "the nerves of government," about the meaning of variations in the ways that organizations receive, process, and respond to information. When he asked his students about the difference between refrigerators and chickens, he was not looking for the observation that you cannot unplug a chicken. Rather, he was making the point that even chickens have rudimentary cybernetic capabilities enabling them to adapt their behavior to their environment. Martin, unhappily, confirms that during the accident at Three Mile Island the responsible agencies of the U.S. federal government behaved more like refrigerators than like chickens. His story is one of the total failure of the Nuclear Regulatory Commission, and other agencies as information-processing entities. This failure was so complete, so utter, and so consistent that Martin's account is hilarious at the same time that it is deeply troubling.

About half-way through the book, I began to underline phrases and verbs with a recurrent theme: "did not believe," "did not know," "garbled," "did not know how," "could not find out," "no answer," "misquoted information," "misinterpreted data," "could not get through," "invalid assumption," "inaccurate data," "mistakenly understood," "was not aware."

The result was that "the federal actors in charge helped turn a crisis into a new calamity."

For several years, Tony Oettinger, Director of the Harvard University Program on Information Resources Policy, has been warning of the risks of concentrating on the "bones and muscles" of government and society while ignoring the nervous system. Martin's account of the events at

Three Mile Island is a vivid demonstration of the urgent need to listen to this warning.

Many readers will, perhaps, be frustrated that Martin's book does not so much end as it merely stops. Its brevity and overall tone of haste is probably partly due to the author's understandable desire for timely publication of research with high contemporary interest. No doubt it is also partly explained by his understanding that the accident at Three Mile Island by no means ended in April 1979, or in 1979 at all, or even in 1980. The melodrama on the Susquehanna River continues into 1981, and by most recent estimate will go on for another *five years.*

I urge Martin to return to the scene and, in a more leisurely manner, to assess whether the Nuclear Regulatory Commission and other responsible government organizations have indeed learned some lessons and are behaving with, at least, the cybernetic capacity of chickens.

IRVIN C. BUPP

Harvard University

The Cost of Good Intentions: New York City and the Liberal Experiment, 1960-1975. By Charles R. Morris. (New York: W.W. Norton, 1980. Pp. 256. $14.95.)

While neo-conservatives vie with each other in disparaging the quest for social justice, Charles R. Morris lends himself to the debate. The title of this book seems to recall Samuel Johnson's observation that "the road to hell is paved with good intentions" and Carter Glass' allegation that "a liberal is a man who is willing to spend someone else's money."

New York City should not have to apologize for the fact that its heart was bigger than its purse. An estimated 70 percent of all Americans or their ancestors came through this port of entry and benefited from its educational system, housing stock, job market, health care, transit facilities and social services. Now, safely ensconsed within the establishment, some who profited from the city's humanitarianism denigrate efforts to guarantee opportunities for new generations.

Morris, former Assistant Budget Director for the city and current vice-president of Chase Manhattan Bank, has written a compact chronicle of the origins, evolution and aftermath of our nation's most traumatic municipal crisis. In 1975, New York City ran out of money. It was unable to repay loans that became due and simultaneously maintain normal operations. The narrative surveys state and federal rescue efforts, creation of the Municipal Assistance Corporation to stretch out short-term debt, and establishment of the Emergency Financial Control Board which placed the city in virtual receivership.

Morris advances the thesis that the fiscal crisis could have been avoided if officials had cut back on spending years earlier instead of borrowing to the brink of bankruptcy. He sharply criticizes the ineptitude of local politicians, the demands of municipal unions, and the inclination of voters to elect candidates who promised the greatest litany of services. Over the years, demographic changes altered the ethnic and socioeconomic composition of the city and eroded the tax base. In the end, hapless officials lost control and failed to comprehend the catastrophe that engulfed them.

This is a tale of three mayors. The vignettes may be useful for a primer on city politics, but contain few original insights. Mayor Robert F. Wagner is portrayed as a well-intentioned executive whose indecision caused him to drift from crisis to crisis as he proved incapable of responding to changes buffeting New York. John V. Lindsay emerges in the familiar garb of puritan and moralist who plunged into warfare against power brokers but contributed to the crisis by failing to retrench after the economy declined. Abraham D. Beame, a former comptroller, knew the gravity of the problem but overestimated his ability to survive the storm. Ultimately, Beame's budgetary legerdemain and public squabbles with his comptroller—an institutional dilemma in the city—undermined investor confidence in the administration.

Morris maintains that New York's ordeal had nationwide ramifications for state and local borrowing, and threatened market stability at home and abroad. To this extent, the book contains material of interest to students of intergovernmental finance. Valuable sections focus upon dubious fiscal practices, e.g., underestimating expenditures, inflating revenue, skipping reserve deposits, shifting accounts from one year to another, alternating between cash and accrual systems, borrowing against uncollectible receivables, and transferring expense items to capital budgets.

Without condoning municipal mismanagement, the reader may legitimately question the extent to which Morris absolves the financial community of responsibility. Bankers who blew the whistle on New York's mounting difficulties or refused to recommend city securities to their clients were not innocent bystanders. They had reaped profits from the city's easy borrowings. One looks in vain for any acknowledgment that the bankers signaled a "run" on the city to lighten burdens on business or used the fiscal crisis as a load-shedding device to force New York to curtail services to its sprawling, pluralistic population.

The scholarship is uneven. Morris concedes that he was not involved in most of the events and directs the reader to "official records or other standard sources." Unfortunately, he has failed to compile a formal bibliography. Journals of major public bodies (e.g., City Council and Board of Estimate) have not been used. The index contains only passing reference to the city's unique Board of Estimate which shares decision-making powers with the Mayor. Morris has relied on newspapers rather than the readily available statutes for provisions of the state's emergency legislation. The account of the rescue effort is surprisingly sketchy. The 1975 city charter, which mandated fiscal reforms, is largely overlooked. Factual errors occur. Governor Nelson Rockefeller's resignation in December 1973, not his acceptance of the vice-presidency a year later, elevated Lieutenant Governor Malcolm Wilson to the executive mansion. Elsewhere, the author confuses the name of the Brooklyn Democratic chairman who opposed Mayor Wagner's bid for a third term. Value judgments abound. Morris cites the intelligence of budget directors who employed him but overuses the epithet "martinet" when describing three commissioners with whom he did not work.

This is not a definitive sequel to Wallace S. Sayre's and Herbert Kaufman's *Governing New York City* (Norton, 1965), which was far more clinical and comprehensive than the present book. As banker and budgeter, Morris views recent city history primarily in terms of management and finance. He never examines the impact of the fiscal crisis upon the people or the ramifications in the loss of home rule. He appeals to those who inhabit corporate board rooms rather than the sidewalks of New York.

LEONARD P. STAVISKY

Columbia University

Subliminal Politics: Myths and Mythmakers in America. By Dan Nimmo and James E. Combs. (Englewood Cliffs, N.J.: Prentice-Hall, 1980. Pp. xiii + 256. $10.95, cloth; $4.95, paper.)

This is a lively (even breezy: the title of chapter 7 is, "Brother, Can Your Paradigm?") review of a topic that is receiving increasing attention in American government studies. It covers much ground in a perceptive way. But its intent is uncertain: is it a simple *exposé* of myths and mythmakers in America, or is it a critical analysis of an important, functional element in the American political system?

The book's topic is not "what people know about American politics." It is about "what people *think* they know about American politics." Specifically, the topic is the "subliminal" element in American politics, "the taken-for-granted set of assumptions, conceptions, and ideas about American politics that lies below the threshold of consciousness, that substantial realm of dramatic accounts whose accuracy and plausibility go largely unquestioned" (p. xii).

But what if, as this book intends to force us to do, they get questioned? Is a consciously believed myth, one that is no longer subliminal, still a myth? This question is not satisfactorially dealt with in the book's central arguments.

The book has two parts. Part 1 defines myth, in the light of the literature on the subject, and identifies the content of America's founding myth and its many contemporary myths. This part shows the essentiality of myth to social organization and how the accretion of it over time has given Americans a "usable past" by which to identify and feel proud about themselves and their country. But this part also systematically shows that much of this "usable past" is contrived; at least it is embellishment of history, at worst it is imaginary history. Throughout, the treatment argues nearly explicitly that mythic thinking, however socially useful, is to be compared unfavorably to scientific thinking.

Part 2 carries forward this ambivalent but generally unfavorable view of myth in politics. In a series of chapters, it examines the myth-making of politicians and their aides, the purveyors of popular culture in the entertainment industry, reporters and TV commentators in the news media, and political scientists. Although many of the points are obvious, this is a useful survey and the last chapter is as particularly interesting, as readers of this *Review* might expect. For it begins by asking, would not political scientists, "above all groups, be immune from the disease of mythifying?" (p. 190). The answer is "simple but severe: Political science, a learned discipline, makes myths" (p. 220). The best part of the chapter is on political science as a service industry to the community.

The chapter ends with the question, can political science demythologize itself? The authors would put that question to the community at large as well. Their answer to it is that, because of human limitations, we cannot live without myth altogether, but we can come measurably closer to that ideal by elevating scientific modes of thought as best we can and, above all, by living with uncertainty, by being willing to live without the certainties that uncritical myth provides.

This answer fudges the central question: what does myth do? If, as I suspect, myth gives meaning—which no amount of "science" can—then "demythologizing" political science, America, or whatever is not the issue. The issue is, which myth should we believe and why?

H. MARK ROELOFS

New York University

Presidential Secrecy and Deception: Beyond the Power to Persuade. By John M. Orman. (Westport, Conn.: Greenwood Press, 1980. Pp. xv + 239. $22.95.)

John Orman is concerned that political scientists have been unwilling to deal with the problems of covert use of power in a theoretical manner that would permit both a method of evaluating presidential acts and a guideline for future presidents. His attempt to fill the gap is an important addition to the meager literature on the subject for it does suggest why presidents in different eras have felt it necessary to resort to secrecy and deception. Unfortunately, the book is less successful in outlining an evaluative scheme or usable guidelines.

Orman divides the reasons for presidential deception into four categories, defined in detail: personality, bureaucratic, pragmatic, and institutional. Using them, he examines Kennedy's program to undermine the Castro regime, Johnson's secret war in Laos, Nixon's actions against the Allende regime, and Ford's covert funding of the FNLA and UNITA in Angola. His analyses deserve to be read; for the most part, they are persuasive. One of the points he makes without being aware of it, however, is that it is not only the personality of the president that can be important to an analysis. Orman writes about the central role of Henry Kissinger in both the Nixon and Ford examples, but says that personality was not a factor in the Angola decision because Ford's was a "normal, well-adjusted presidential personality type" and he had "the final say" over foreign policy (p. 155). Without joining the current movement to equate Kissinger with Rasputin, it is still possible to raise the question of the extent to which any president, however mentally sound, is dependent upon his advisors in various areas, and to what extent his final control of foreign-policy decisions can negate the impact of an advisor's advice and personality. No president can be better than his information system; no information system is personality-free. Kissinger's personality may have been as important to Angola as Ford's.

Orman's analytical categories work well, even if one might quarrel with specific applications. What he meticulously describes as his "value framework" does not. Orman argues that illegal, unconstitutional, or unethical acts are never justified, but may be necessary; that means and ends must be compatible; that the president should tell the truth whenever possible; and that he should favor open over secret alternatives. The difficulties here should be obvious. One example: Orman, assessing the four examples of presidential deception according to this value framework, concludes, amazingly, that Ford's covert Angola policy was acceptable: it was not illegal, unconstitutional, or unethical. He admits that Ford "was not behaving consistently with the intent of the congressional measures that required consultation with Congress"; nonetheless, Orman finds Ford's actions ethical because Ford "believed he was acting consistently with his responsibility . . . his duty . . . and his discretionary power" (p. 185). That makes nonsense of any attempt to delineate limits to presidential secrecy and deception. The ability of presidents to delude themselves about the ethics of their behavior must be evident to anyone who has read through Johnson's or Nixon's memoirs.

The ability of political scientists to delude themselves that all political action can be segmented into neat scientific categories is equally great. Orman's suggested guidelines for presidential behavior are admirable. However, just as the generalized language of laws cannot cover all situations, so also Orman's or any other criteria cannot foresee all possible circumstances or be used as strict guidelines for presidential action; the interpretation and application is all. Were presidents to follow the *spirit* of "Orman's law," there is no question that there would be less presidential secrecy and deception. The same result would be achieved, however, if presidents followed the spirit of the Constitution.

PHILIPPA STRUM

Brooklyn College, City University of New York

State Income Taxation. By Clara Penniman. (Baltimore: Johns Hopkins University Press, 1980. Pp. xiii + 292. $22.50.)

It has been 20 years since Clara Penniman and Walter Heller wrote their first volume on the administration of state income taxation. In the interim, studies of tax progressivity, income redistribution and tax efficiency have littered the literature on state income taxation. Penniman's latest work on state income taxation is a relief from these often repetitive and narrow economic studies. Returning to more solid ground, Penniman directs our attention to the subtle effects of organizational structure on the operation of state income tax.

Penniman sets out to examine the organizational structure and operation of state income tax offices. Maintaining that organizational capacity structures administrative efficiency, Penniman presents evidence that confirms the wide variation in structure, management, processing and auditing operations of state income taxes. The text begins with a historical description of the state income tax, establishing its fiscal and political importance to the operation of state government. A general typology of state tax administrative structure is outlined with the degree of structural integration as the central organizing concept. The virtues of integration and its accompanying benefits for the operation of tax systems are not overstated. Penniman notes that "over integration may defeat the values of specialized experience and knowledge that can produce a needed overall view of administration" (p. 100).

The remaining chapters examine the specific tasks associated with the operation of a state income tax. The organizational typology is applied as a means of making comparisons across states employing an income tax. At each stage of the process Penniman offers provocative explanations for the breakdowns and inefficiencies in the tax system that go beyond traditional cost-benefit analysis.

A separate chapter on intergovernmental relations underscores the reliance states must place on each other and the federal government in order to achieve an effective income tax. In spite of more conflictual state-federal relations, Penniman concludes that there is greater federal-state than interstate cooperation in the administration of tax collections. Presumably the federal government's dominance of the income tax forces states to rely on the Internal Revenue Service and its superior collection and auditing procedures. This issue is not fully explained and remains the only major weakness in an otherwise thorough review of intergovernmental relations and tax administration.

Students of public administration and public finance should find this text a primary source for the study of state revenue systems. There is, however, a potentially broader audience for this book. Policy analysts and innovation theorists should find Penniman's historical and comparative analysis on the evolution and diffusion of state income taxes enlightening. Penniman's own data suggest that adoption of income taxes—frequently studied by others as a policy innovation—has often been a function of revenue dependence (i.e., percent of total revenues raised from income taxes) and existing administrative structures.

Penniman's only failing is her reluctance to analyze fully her own questions and hypotheses. In many instances she avoids quantitative analysis of her own tables. The attentive reader, however, will note that many of the tables provide ample opportunity for further analysis. When examining the variation in the administrative structures of state income taxes, Penniman ignores the tables which show a regional (i.e., southern and southwestern) bias toward the use of integrated administrative structure.

Penniman presents a clearly written and well-documented description of income taxation in the American states. This volume outlines a research agenda which should occupy scholars for another 20 years.

ROBERT STEIN

Rice University

The Electorate Reconsidered. Edited by John C. Pierce and John L. Sullivan. (Beverly Hills, Calif.: Sage Publications, 1980. Pp. 293. $18.95, cloth; $9.95, paper.)

Need it be said for the hundred-thousandth time that the papers in a collection such as this vary in quality? The present volume contains—out of 13 papers and essays—a couple of choice ones and several more which are solidly informative and useful. It is introduced by the editors' necessarily sketchy review of the mainstream of voter behavior literature.

Pamela Johnston Conover's paper on "The Perception of Political Figures" is rather marred by obviousness. She shows that people who were convinced that Nixon *was* involved in Watergate came to believe that he was the sort of person who *would* engage in that sort of activity. She tries to give theoretical import to her finding by the use of something called "attribution theory." Unfortunately, the predictions she deduces from attribution theory are either inconsistent with the data or identical with the predictions more conventional theories would produce. For example, she finds that those who were already most convinced of Nixon's culpability were the most attentive to media coverage of Watergate; a classic example of that old social-psychological warhorse, selective exposure.

Paul R. Hagner and John P. McIver tackle the question of response instability of survey respondents—the familiar "attitudes versus nonattitudes" problem of Converse, Achen, et al. They bring to the debate, happily, a new set of panel data, and an interesting suggestion: many "unstable" patterns of response are consistent with real attitude change. For example, a respondent may agree, agree, agree, and disagree over four waves of a panel. He or she may have simply changed his mind. However, the respondent also may be answering randomly. Hagner and McIver never properly deal with this latter possibility, and their data are quite consistent with a "black-white" model where 25-45 percent of respondents actually had an opinion, and the rest responded at random.

James Piereson, John L. Sullivan, and George Marcus reevaluate the classic findings on political tolerance by measuring intolerance, not merely toward left-wing groups, but toward right-wing groups as well. They show that earlier studies were in error in important ways—greatly overestimating the relative tolerance of the educated, city dwellers and non-Protestants.

In contrast to the psychological orientation of most of the essays in the volume, Herbert F. Weisberg's and Morris Fiorina's is an important contribution to rational choice theory. Such theory has usually posited that voters see candidates as occupying a single position on an ideological continuum—candidate A is seen as a moderate conservative. But suppose voters lack good information on candidate positions, and must view any candidate as a probability distribution—candidate A is likely a moderate conservative, possibly a moderate liberal, and just barely possibly a socialist? Orthodox theory suggests that voters would view such a situation as a lottery and vote for the candidate offering the largest expected payoff. Weisberg and Fiorina suggest another possibility: voters may favor the candidate offering the smallest probability of some unacceptable outcome. This theory of "defensive voting" is of course, an application of the "minimax regret" decision criterion. A silly contrivance when applied to voting turnout, the theory has plausibility here. Perhaps Goldwater lost, not because he advocated unpopular policies, but because he offered a perceptible probability of a nuclear war. The authors' attempt to test this theory, though ingenious, fails, partly because nobody has collected the appropriate data and partly because for the great majority of respondents this and more traditional theories yield identical predictions.

John Petrocik's concluding essay deals with the important controversy surrounding the "revisionist" view of the electorate presented in *The Changing American Voter.* As a co-author of that volume, Petrocik would be expected to rebut criticisms of it. He does so ably, and clarifies several issues in the process. Only a couple of methodological blunders mar the piece. Petrocik attempts to assess issue voting by regressing the vote on party identification, and then looking for an ideological pattern in the residuals. Elsewhere, he analyzes variance in the vote contributed by party identification, variance contributed by issues, and "shared variance"—the latter being a nonsensical category.

Space allows only the tersest mention of several other essays. Christine B. Williams considers problems of cohort analysis, particularly the difficulty of separating out cohort, aging, and period effects. The methodologically sophisticated will find little new here; others may find this a worthwhile introduction to the subject. Conover and Stanley Feldman deal with the question of ideology or "constraint" among issues. They argue that—rather than a single liberal-conservative continuum—multiple *schemata* serve to organize specific attitudes. They demonstrate, admittedly in a preliminary way, the utility of schemata in analysis of 1976 CPS data. W. Phillips Shively contributes a most valuable review of the literature surrounding the question: what does it *mean* to call oneself a "Democrat," "Republican," or "Independent"? Edward G. Carmines and James A. Stimson analyze racial attitudes, and show that they are very stable relative to other attitudes, and central to any structure of ideological constraint among the public. They also show a modest but significant realignment of party identification around racial issues in the 1960s.

Voter behavior specialists will find this volume worth the purchase price (in paperback). Others will benefit from selective reading herein.

JOHN MCADAMS

Marquette University

Presidential Decision Making: The Economic Policy Board. By Roger B. Porter. (New York: Cambridge University Press, 1980. Pp. xi + 265. $18.50.)

This book is a case study focusing on the structure and operation of an advisory body during the Ford administration, the Economic Policy Board (EPB). Roger B. Porter, formerly executive secretary of the board and presently an assistant professor of public policy at Harvard, chronicles its history, notes its functions, and details its organization. He presents three controversial policy issues (the 1975 State of the Union tax proposals, the U.S.-USSR grain agreement, and the 1976 footwear import decision) to illustrate the operation of the board and provide criteria for evaluating its performance.

But the book is also more than a case study. While it focuses on the EPB, the author is concerned with advisory processes in general. He places the EPB within a conceptual framework and even amplifies that framework in a concluding appendix on processes for decision making. Porter's objective, broadly conceived, is to examine and assess these processes in the light of the fragmentation of the executive branch and the interrelationship of policy issues.

He is successful. He has written a detailed institutional history, an insider's account of who said what to whom, when, and with what effect. Most of the book consists of a nuts-and-bolts treatment of the EPB. Its meetings, agenda items, even policy papers, are noted and illustrated with numerous examples. Roles of the principal actors are discussed and evaluated. The president's involvement is indicated and his economic decisions seen within the context of EPB advice. Porter describes an active, formal, systematized structure, one that operated to exchange information, to coordinate advice, to resolve interagency disputes, and most significantly to provide the president with a variety of policy recommendations within the economic sphere.

The description is rich but almost burdensome in detail. It is also curiously antiseptic. Very little of the excitement of presidential decision making is conveyed. On the contrary, the board's attempts to coordinate conflicting positions read as if they were sanitized (much as Ford's own memoirs read).

If judged by the performance of basic advisory functions, Porter believes "the EPB was a success" (p. 212). He argues that it provided the president with an early warning system, helped define critical issues and focused administrative attention on them, worked to mobilize information and analyze policy, and presented a range of options for decisions. Most importantly, it did so within an advocacy system characterized by due process. In the light of the previous administration's less systematized and more hierarchical economic advising structure, Porter suggests that this was no small accomplishment.

The weaknesses of the board are mentioned, but quickly dismissed. The time spent on trivial matters, the delays in reaching policy judgments, the undercutting of cabinet secretaries in their areas of responsibility, are seen as minor problems. Porter concludes with an unqualified endorsement for the board from the president and his aides:

> Gerald Ford described it as "the most important institutional innovation of my administration." Indeed, the EPB not only had the reputation of being the most effective policy council in the Ford period, but also the most sustained, comprehensive, and successful collegial attempt ever to advise a president on economic policy matters (p. 212).

Porter and his colleagues, of course, cannot be considered impartial observers and judges. As executive secretary of the EPB from 1974 to 1977, Porter contributed significantly to its daily operations. The extent to which his service on the board affected his own observations and evaluations and his interviews with other key participants is difficult to assess from the outside. What is clear, however, is that despite the outstanding grades which the Ford administration gave its EPB, President Carter chose not to continue it, although, like Ford, he too desired a cabinet approach to policy making; he too wanted advocacy with due process.

Roger Porter has written a valuable work for students of the presidency. It is conceptually sophisticated and empirically useful. Scholars should benefit from it regardless of whether they accept the author's appraisal of the board's work or his prescriptions for organizing economic advice to the president.

STEPHEN J. WAYNE

George Washington University

Case Selection in the United States Supreme Court. By Doris Marie Provine. (Chicago: The University of Chicago Press, 1980. Pp. ix + 214. $18.00.)

Doris Marie Provine has written one of the most authoritative accounts to date of the process through which the justices of the U.S. Supreme Court decide whether to consider cases on their merits. Considering both macro- and micro-level implications of these important decisions, she exhaustively and creatively exploits the unique data set created by Justice Burton's compulsiveness. And because it is impossible to doubt the importance for legal and public policy of this stage of the decision process, her work stands as an important contribution to the understanding of policy making by the Court.

In the early chapters of her book Provine provides an historical review of the efforts of the Court to maximize its discretion over deciding which cases to hear. Basically she argues that the Court has purposely adopted ambiguous standards for case selection (e.g., Rule 19) in order to avoid any constraints on its discretion, and to reinforce diffuse support for the institution by maintaining the myth that citizens can ultimately have their day in the highest court in the land. A latent effect of this ambiguity, however, is the substantial advantage that accrues to what Galanter has called "repeat players." Great discretion also allows the Court to manage cases which might otherwise threaten the prestige or political stability of the institution, as, for example, her analysis of major race-discrimination cases from 1946 through 1957 reveals. The historical and logical analysis of the first two chapters of the book make interesting reading.

In chapters 3 through 5 the author considers the determinants of case selection decisions. The Burton data are used to reexamine the utility of cue theory (as developed primarily by Tanenhaus and Ulmer). Provine basically rejects cue theory, arguing that it cannot accommodate the finding that factors determining the decision to "special list" a case also control voting decisions after discussion. In its place she argues for a process of weighting the "salient aspects of all petitions" (e.g., case subject matter and litigant expertise). Most important, however, seems to be the justices' shared ideas about the proper work of the Supreme Court. "A justice who stresses the obligation of the Supreme Court to correct lower-court mistakes, for example, will vote for review when a justice with the same interpretation of the results, but a different view of the responsibility of the Court, will not. . . . Even the shared goals for Supreme Court adjudication . . . can occasion disagreement in case selection because the justices can be expected to differ in how they evaluate specific cases in light of these goals" (p. 105). Provine divorces the propensity to vote for review from the ideology of the justice, arguing instead that the propensity flows from role conceptions or orientations. Judicial attitudes affect these votes only indirectly. The primary conclusion of her analysis is adequately summarized as follows:

> The Burton data suggest that Supreme Court justices are not ordinary political decision makers, because members of the Supreme Court share a conception of their role which prevents them from using their votes simply to achieve policy preferences. Thus we have seen that none of the Burton-period justices either combined forces to achieve the results they preferred on the merits, or voted individually in a way that would suggest routine calculation of probable outcomes in the case selection process. Nor did the Court as a whole appear to make the calculation of political harm to itself central in even the most politically volatile case selection decisions (pp. 174-75).

One can quibble with portions of the analysis, of course. Her statistical analysis and procedures are seldom rigorous, and the unit of analysis is not always optimally suited to addressing the issue under consideration. Too little attention is given to the matter of the relationship between role orientations and ideology. As role orientation is the major independent variable in her model, and as variability exists, some discussion of the origin of these beliefs seems in order. The implications for generalizability of the uniqueness of the data base, the limited time period analyzed, and the restricted range of major environmental variables have also been ignored.

More importantly, while the empirical analysis of this important data set is descriptively interesting, little theory is developed or tested. For instance, the author skirts the boundaries of role theory on several occasions but is never quite willing to consider a full-blown role-theory treatment of her subject (and this despite her belief that "the self-imposed limits of role conceptions" constitute one of the most important parameters of the model). Similarly, while her analysis of the manipulation of symbols by the Court is insightful, much would be gained by placing the analysis within the context of theories of support. The analysis of decisions by justices ignores the complex and theoretically sophisticated model developed by Ulmer. Indeed, little attention is given to Ulmer's recent work. Provine considers, for example, the question of strategic or outcome-oriented voting in FELA cases from the perspective of Schubert's early 1960s game-theory approach rather than from Ulmer's more recent (e.g., *APSR,* 1978) and more subtle theory. More self-conscious attention to theory would have substantially improved the analysis. As a consequence, these data will only fully be exploited as they become the grist for another theoretician's mill.

As a handy descriptive reference work on access decisions, this book will be useful to many. As a theory of judicial decision making, its utility is somewhat limited.

JAMES L. GIBSON

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Old Folks at Home. By Alvin Rabushka and Bruce Jacobs. (New York: Free Press, 1980. Pp. vi + 202. $10.95.)

Adequate housing ranks along with income security, nutrition, health care and physical safety among the basic needs of this country's old-age population. A number of government programs have been developed to assist elderly people in meeting their housing needs, and yet for the most part policy in this area has evolved with no very substantial empirically based understanding. There has been only limited knowledge of the actual physical status of housing for the aged and little grasp of how old people themselves feel about their residence and its physical condition. Leading authorities in the housing field have underscored the scarcity of data concerning how most elderly people live and their living arrangements, and have worried over the tendency for policy to be decided essentially on the basis of conjecture. The knowledge gap probably has contributed to the absence of notable success in government housing policies for the elderly.

Compounding the difficulty has been a tendency for certain of the more widely read books in the field of social gerontology—including at least one leading textbook and a Pulitzer Prize-winning book intended for general readers—to come dangerously close to the assertion that old age is synonymous with poverty. The implication is left that the aged on the whole are also poorly housed, since income levels and housing quality are closely connected.

Against this background, the research findings reported by Rabushka and Jacobs represent a valuable corrective, and their analyses and interpretations amount to what is probably the most balanced picture of the present situation. The authors' interest centers on the 70 percent of elderly Americans who own their own homes. (The tenants who comprise the remaining 30 percent are excluded from discussion.) Their data were derived from a survey of 1,575 older-aged respondents situated in seven communities chosen as reflective of the diversity of the United States in terms of size, region, climate, race and culture.

The findings do not fit easily into either the preconceptions of those on the political right, convinced of the potential of the private market to generate adequate housing for old people at acceptable costs, nor of those on the liberal left, who are inclined to attack housing needs through direct subsidies to elderly homeowners and other governmental assistance based on an age criterion alone. The "free market" proponents will have difficulty reconciling their stance with Rabushka's and Jacobs' finding that fully 60 percent of elderly homeowners live in a residence with at least one significant "system defect," meaning a lack of either (1) some basic facility such as a toilet or working stove, or (2) a significant health or safety hazard, or (3) some long-term repair need such as a ceiling or wall fixture in major disrepair. Even if one confines attention to the second of the above categories, namely hazardous and dangerous features that directly threaten the safety of occupants or render the house uninhabitable, it was found that 40 percent of surveyed homes had at least one defect of this sort. Many had two or more. On the other hand, liberals will find it sobering to contemplate the authors' findings regarding the resistance of most elderly homeowners to direct government aid or subsidy, given the latter's fierce sense of pride and independence. Notwithstanding the prevalence of serious "system defects," elderly homeowners are satisfied with their house (96 percent), have no desire to leave it for a condominium offering more amenities and no structural defects (93 percent) and are content with its size, even though the current household may consist of only one or two people occupying a dwelling large enough for four, five or more. Even where serious defects exist—including ones directly endangering health and safety—elderly homeowners express decided satisfaction with their residence, being either unaware of the existence of the defect or not perceiving it as a serious impediment to their living arrangement.

This lack of congruence betwen objective reality and subjective feelings on the part of homeowners—termed by the authors as the "needs/preferences mismatch"—carries with it a major public policy challenge. Rabushka and Jacobs are not opposed to government housing programs of all kinds, but they are disturbed by the premise that the gap between accepted standards of housing decency and the elderly's actual housing situation can be filled only by direct public intervention and regulation. They see the government's proper role as one of facilitator—aiding homeowners to perceive housing defects where they exist and then enabling them to correct problems with property tax deferrals, "reverse annuities," government-underwritten loans, and so forth.

Particularly disturbing is the image presented in the book of distress among elderly black homeowners. While a number of factors—age of structure, and homeowner wealth, income and health status—are revealed as moderately associated with the presence (or absence) of critical structural defects, these pale in significance compared to homeowners' race. Black homeowners have four times the number of critical structural defects in their houses as whites; 95 percent of homes owned by elderly blacks have at least one critical defect, as against only 44 percent among their white counterparts. Black/white income differentials obviously account for some of this disparity, but less so than one might expect. "White" houses are better maintained, even with income held constant. The authors speculate that black homeowners still suffer restrictions and discrimination in their ability to buy repair and maintenance services, and also that differences in taste or preference along race lines may be involved.

The book is not without its flaws. As noted, Rabushka and Jacobs avoid any treatment of housing conditions among the 30 percent of the elderly not in the homeowner category. From the standpoint of understanding the housing needs of older people generally, this gap in the evidence represents no small problem, especially since earlier research has revealed that renters and homeowners comprise distinctly different categories of the elderly population.

There is also an unsettling difference in the general mood conveyed by the book's opening two chapters and that of the closing three. The opening section strongly emphasizes the need to challenge so-called "experts" in their view that old-age dependency and severe housing needs are more prevalent than is commonly recognized. Against this, the authors assert that "the touting of crises create[s] an impression of a large number of problems and current needs of the elderly. But this impression applies only to a small number of the elderly and an even smaller number of homeowners" (pp. 12-13). And again, in remarking on the dismal image conveyed in the mass media of elderly people confined to "rural shacks, nursing homes and overcrowded hotels," the authors affirm that "elderly homeowners, in particular, enjoy a standard of living and personal independence unknown to prior generations" (p. 33). All this does not quite prepare the reader for the subsequent disclosure that, contrary to observers who maintain that "America has almost met the goal of a decent house for all Americans" (p. 92), the actual extent of deteriorated housing among the elderly is rather high as measured by professional standards of housing quality. Even though many of these defects are not perceived as problems by homeowners, they are nevertheless *real* problems, both for the individuals involved and for local communities. As the authors remark somberly toward the end of the book, "A deteriorating stock of elderly-owned housing *does* raise serious issues for national housing policy involving not only the well-being of the elderly but also affecting millions of their tax-paying neighbors" (pp. 131-32).

Despite the flaws, however, Rabushka and Jacobs have made a useful contribution to the public policy literature on housing. Their book will be read with special interest by those interested in the social and economic needs of the elderly and in what lawmakers and public officials can do to alleviate those needs.

HENRY J. PRATT

Wayne State University

Congressional Odyssey: The Saga of a Senate Bill. By T. R. Reid. (San Francisco: W.H. Freeman, 1980. Pp. xi + 140. $4.95.)

The legislative case study is a potentially important but not particularly fashionable genre of literature for contemporary congressional scholars. One can count on the fingers of two hands the number of such first-rate case studies since World War II. These studies tend to be explicitly atheoretical, although they are replete with the raw materials useful for the development of broader verbal theories of the legislative process. T. R. Reid's volume is a small gem which should be received gratefully by congressional scholars and their students, for it represents a fine addition to the case study "tradition." The book is the polished end-product of a series of *Washington Post* articles during the 95th Congress which were intended to educate readers on the intricacies of getting a rather unglamorous piece of legislation enacted. At first glance, there would seem to be nothing captivating about Republican Senator Pete Domenici's struggle to establish a waterways user charge upon the U.S. barge industry; the bill in one form or another had been a doomed "hardy perennial" for decades. But first glances are often misleading, and as Reid tells the tale it becomes enormously fascinating and instructive.

Among the themes which receive skillful treatment in the study are the importance of strategy, the crucial role of staff, the interplay of personalities (best illustrated by certain of Domenici's actions and the extraordinary maneuverings of Senator Russell Long), the triangular relationships among legislators, executive branch personnel, and interest-group representatives, the occasionally baffling exercise of parliamentary ploys and prerogatives, the nature of committee and party leadership, and—not least—the force of ideas. Although Reid never brings together the pieces of the legislative process puzzle in a consciously theoretical manner, his narrative is so clearly focused and judicious that even a relatively unsophisticated reader will emerge with a good sense of how legislation gets made. I know of no other book of its ilk which so adeptly orders congressional complexity in so short a space. For this reason, its classroom appeal is considerable.

This is not to say that the book is unflawed. In spots it gives us a quick shuffle on technical and parliamentary matters, and it occasionally avoids explanation or speculation about participants' motives or strategies. Readers who have had the opportunity to be participant-observers on Capitol Hill may well conclude that Reid somewhat underplays the tedium, frustration, and deadlock which can characterize the struggle for passage. However, one never gets the sense that important chapters in the story have been missed.

Another issue which deserves mention is the possibility that Domenici's bill is a case study in the operation of a "Hawthorne effect." The extensive *Post* coverage of the legislation vaulted what might otherwise have been an obscure bill into a position of much higher priority on the agenda of official Washington. We can only wonder how different behavior and outcomes might have been if the newspaper had not anointed the waterway user's charge bill as one worth following.

This book shows that there may be real benefits to be derived from future collaborative efforts between journalists and political scientists in the execution of congressional case studies. Only through the disciplined accumulation of case study evidence drawn from a variety of types of legislation will it be possible to generate a body of grounded theory. And only by generating such theory will we come to know why Congress makes policy as it does. Whether the interests and talents of journalists and scholars can be made to mesh remains to be seen.

CHARLES M. TIDMARCH

Union College

The Politics of Privacy: Planning for Personal Data Systems as Powerful Technologies. By James Rule, Douglas McAdam, Linda Stearns, and David Uglow. (New York: Elsevier, 1980. Pp. xii + 212. $14.95.)

Personal privacy is an elusive albeit primary value in a free society. In the twentieth century, privacy emerged as an important issue in American politics, reflected in mounting litigation and legislation. Although the Constitution does not expressly guarantee a right of privacy, the Supreme Court broadly interpreted the First, Fourth, and Fifth Amendments to safeguard some privacy interests. More dramatically, in a herculean exercise of judicial activism in *Griswold* v. *Connecticut* (1965), the Court fashioned an independent constitutional right of privacy. The denomination and subsequent expansion of the right under the Fourteenth Amendment invited further litigation, as well as engendered considerable controversy over the nature of privacy per se and the basis for, and the necessary limits to, claims of a constitutional right of privacy. The growing concern over achieving protection for personal privacy in a technological and information-oriented society also prompted congressional and presidential action. Numerous study commissions were established and Congress enacted the omnibus Privacy Act of 1974, along with other legislation designed to safeguard citizens' informational privacy interests in financial, medical, educational and criminal records. The political salience of safeguarding privacy interests was further registered by the increasing number of states adopting privacy statutes.

James Rule and his colleagues, all sociologists, purport to give a comprehensive examination of the privacy controversies arising over public and private organizations' reliance on computerized information systems. They also offer a critique of the incremental development of federal privacy protection politics and, thereupon, an alternative proposal *à la* solution to the dilemma of preserving privacy in an information-oriented society. Unfortunately, this authors' description of the informational privacy controversy is shallow and their prescriptions neither well-thought-out nor particularly illuminating.

The authors' discussion of the emergence of privacy as a political issue is analytically and descriptively weak. The authors attribute the controversies in the 1960s and 1970s to the social structure of an advanced industrial society in which complex organizations place a premium on gathering personal information about their clients. Beyond such sweeping generalizations, however, the authors fail either to ground their analysis in the distinctive features of the American political process or systematically to address the dynamics of the formulation and articulation of privacy protection as an issue worthy of political actors' attention and policy making. No attention is given to the import of privacy litigation—constitutional or other varieties—and the book contains only a superficial discussion of what the authors call "official" responses to privacy controversies. For example, a meager chapter of 2½ pages is devoted to the crucial provisions of the Privacy Act.

A comprehensive and detailed discussion of the development of privacy protection policies would have made a contribution to the literature. Moreover, such a discussion might make plausible the authors' conclusion that further legislation and procedural reforms are inadequate to safeguard privacy interests in computerized record systems. The authors are critical of present policies, as embodied in the Privacy Act, for balancing organizations' information needs with individuals' privacy interests. Indeed, the authors reject outright any balancing approach and find legal, procedural safeguards insufficient, since they are only "matters of social convention" (p. 141).

After a rather cavalier dismissal of public policies designed to accommodate organizations' legitimate information needs and individuals' privacy interests, the authors propose that *"the alternative to endless erosion of personal privacy through increased surveillance is for organizations to relax the discriminations which they seek to make in their treatment of people"* (p. 154, emphasis in original). If organizations were less discriminating in their treatment of individuals, then the demands for personal information would ostensibly drop and thereby enhance the prospects for personal privacy. The authors suggest, for instance, that simpler tax laws would decrease the amount of information required by the Internal Revenue Service and concomitantly diminish its intrusiveness into citizens' private affairs. Fundamentally, the authors are not so much concerned with privacy safeguards in an information-oriented society as dismantling the bureaucratic structure of complex organizations, substituting "rational planning" for incremental policy making, and cultivating what they "term a desireable 'looseness' in social relations" (p. 118).

The Politics of Privacy is a superficial work that neither clarifies the dynamics of the privacy issue in American politics and law, nor offers cogently argued policy alternatives.

DAVID M. O'BRIEN

University of Virginia

The American Establishment. By Leonard Silk and Mark Silk. (New York: Basic Books, 1980. Pp. xi + 351. $13.95.)

The term, the "American Establishment," has been the subject of much speculation over the years, particularly since the publication of Richard Rovere's brief article in the *American Scholar,* which was reproduced in a widely read book in the early 1960s. Yet, curiously, there has been no in-depth analysis of this powerful complex by either serious researchers or more popular writers. Now *The American Establishment,* a study prepared by the *New York Times'* Leonard Silk and his son Mark, has attempted to fill this void.

How well have they succeeded? Not too well, judged by academic standards. This book is basically a journalistic effort which, while well written, relies heavily on an unspecified number of personal interviews. It is consequently a very mixed work. To their credit, the authors focus on selected key components of the American Establishment—namely, Harvard University, the *New York Times,* the Ford Foundation, the Brookings Institution, the Council on Foreign Relations, and, though they are dealt with in much briefer fashion, the Council for Economic Development (CED), the Business Roundtable, and the Trilateral Commission. As a result of their research, the Silks have come up with some revealing insights concerning these important bodies.

But, unfortunately, their treatment of these institutions is generally superficial, often consisting of little more than interesting anecdotal accounts of recent major events. The Silks' book also contains many misleading statements (see page 314 for Richard Nixon depicted as a populist!) and has serious gaps. For example, with regard to Harvard, the authors concentrate primarily on the school's social and religious life and its substantial public service contribution to American government. However, one might ask, what about the role of the Harvard business and law schools as trainers and suppliers of talent to the nation's large corporations? And shouldn't Yale and Princeton have been included as almost equally influential institutions? Similarly, serious questions can be raised about the authors' treatment of the other civic and economic bodies described in this work (for a much fuller and more accurate account of the origins of the CED, see Robert Collins' article in the Autumn 1978 issue of the *Business History Review*).

Another even more grievous deficiency in the Silks' analysis of the American Establishment is the fact that it grossly underestimates the role played in this complex by major corporate interests. The pro-business makeup of such groups as the Council on Foreign Relations is simply glossed over by the authors in their attempt to portray it as an essentially high-minded, though sometimes divided, civic group deeply concerned about the nation's security and diplomatic affairs. No mention is made of the fact that almost all of the men who have served as president of the Council on Foreign Relations in the postwar period have had important corporate directorship ties. (Also, for a broader look at the recent economic links of many of this organization's directors, see the latest edition of Thomas Dye's *Who's Running America?*) And for some reason, the Silks fail to treat one of the often overlooked key arms of the American Establishment, the Business Council (or as it was known up to 1961, the Business Advisory Council). Above all, they do not show how the institutions they describe fit into the overall structure and operation of the American Establishment; each is analyzed as a separate unit. To give just two examples, this book does not delve into any of the links which exist between the Business Council and CED and later the Business Roundtable, or, in the diplomatic realm, between the Council on Foreign Relations and the Trilateral Commission.

Yet another closely related failing in the Silks' work is their distinctly conservative view of the role of the American Establishment as an essential "third force" in the country, one outside of business and government (though with links to both), dedicated to promoting truth, liberty, and, to use that elusive phrase, the "public interest." They also claim, toward the end of their book (see p. 326), that the Establishment has acted as a mediating force between "rich and poor, black and white, management and labor, industrialists and environmentalists, hawks and doves," or, as they put it elsewhere, between capitalism and democracy. This is an extremely naive and misleading position for two supposedly well-informed writers to take, for, although certain large concerns have acted as "outsiders," big business is an integral part—in fact, some would say the very heart—of the American Establishment.

Thus perhaps the best judgment one can make about *The American Establishment* is that while it clearly represents a contribution to the sparse literature on the subject, one can only regret that the authors did not do a better job of analyzing this intriguing topic. (It certainly does not measure up, in terms of economic analysis, to Peter Newman's *The Canadian Establishment.*) In short, a sound study of the American Establishment has yet to be written.

PHILIP H. BURCH, JR.

Rutgers University

The Prospective City: Economic, Population, Energy, and Environmental Resources. Edited by Arthur P. Solomon. (Cambridge, Mass.: MIT Press, 1980. Pp. xx + 491. $27.50.)

Arthur Solomon and his colleagues have provided an up-to-date review of the socioeconomic trends shaping American cities. This is a valuable service, if for no other reason than that nonspecialists now have much useful data analysis conveniently collected in one place. The papers are authored by many of the best-known students working in the urban field, including William Alonso, Thomas Pettigrew, Ira Lowry, Bernard Frieden, Roger Vaughn and George Peterson. The papers by Vaughn and Peterson are particularly interesting as both attempt to establish the considerable impact federal policies have had on urban form and economic vitality. For those who still doubt it, they provide overwhelming evidence that any attempt to explain the significant features of cities, including the policies they pursue, without reference to national politics and policies is not likely to be a rewarding enterprise.

Two other themes, which are the subject of a good deal of present controversy, are also taken up by several of the papers. Among others, Alonso, Lowry and Franklin James consider whether the rehabilitation of older central-core neighborhoods by young professionals will alter the economic and social prospects of cities. All of the authors appear to be suspicious of the more sweeping claims made by editorial writers concerning a comeback for cities (once again!). No one who has ever actually traveled around a larger older American city and observed the comparative size of the "gentrified" neighborhoods ought to be surprised at the critical view taken in these papers. Several of the papers, particularly the one by Harvey Garn and L. C. Ledebur, also illuminate further the question of regional variations in the economic and social well-being of cities. Here again is a warning against unrewarding approaches to the study of cities. The Garn-Ledebur findings demonstrate that significant regional variations exist and any urban analysis must have this as one of its central propositions.

With all the useful data and discussion in this volume, there is one thing missing: politics. Most of the authors appear to be writing of that mythical land where the most serious matters are the policy alternatives to be given decision makers. Whether decision makers have the remotest incentive to listen to the policy proposals or to consider the data on which they rest is not discussed. Moreover, if indeed such decision makers thought it politically valuable to try yet another urban policy, the question must be raised whether it could in fact be sustained over time, given the present institutional and political arrangements. However, behind all this is a more serious question altogether. What do we want cities *for?* Is an urban policy merely one more instrument (or set of instruments) interchangeable with many others (poverty strategies or the latest member of the stable, viz. reindustrialization)? Is the only real question which one is most efficient, since none has any intrinsic significance? In short, are cities, and thus city politics and policies, particular and important elements of the larger regime? If they are, then a volume on urban problems without a discussion of urban politics is a peculiar enterprise at best.

STEPHEN L. ELKIN

University of Maryland

Seabrook and the Nuclear Regulatory Commission: The Licensing of a Nuclear Power Plant. By Donald W. Stever, Jr. (Hanover, N.H.: University of New England Press, 1980. Pp. xv + 248. $15.00.)

As the title suggests, this book is a case study of how one of the most powerful regulatory agencies ever created carries out its responsibilities. As the site of the first serious anti-nuclear opposition to an American nuclear plant, Seabrook, unlike the Three Mile Island (TMI) facility, was controversial before it was built. Like TMI, Seabrook raises important questions about the Nuclear Regulatory Commission's (NRC) powers and procedures.

Donald Stever, formerly the assistant attorney general of New Hampshire, seeks not "so much to indict the NRC as to understand it" (p. xi). He methodically examines the commission's practical problems and legal techniques involved in site selection, financial qualification criteria, public safety, and emergency planning. As such, this treatise is one of the first attempts to analyze NRC decision making outside the polemic of the antinuclear movement. The TMI accident confirms many of Stever's findings and the president's TMI Commission corroborates his suggestions for change.

Stever's thesis is based on "the premise that siting decisions should be made in the public sector

. . . [and] that . . . siting laws . . . which leave issue development to . . . an adjudicatory hearing held long after the utility has invested many thousands of dollars to justify it, are less than adequate to the task" (p. 29). In developing this argument, he traces the controversy from the time of its inception, and it is convincingly demonstrated that the NRC routinely licenses plants on extremely thin financial, safety, and environmental evidence.

Seabrook and the Nuclear Regulatory Commission shows, for instance, that neither state nor National Environmental Policy Act review has a significant impact on the choice of sites or the range of alternates considered. Further, it is pointed out that vendor safety information is taken on faith since the agency lacks capability to make independent evaluations. It is also useful to learn from an authoritative source that the time consumed in the licensing process is the result of the NRC's inefficient way of doing business, not the product of environmentalist delay tactics. Since the Seabrook decision presents an ominous precedent for the future, Stever hopes that it will become a watershed leading to a more responsible regulatory program for the simple reason that "the NRC does not undertake its role as a regulator with adequate objectivity, . . . does not listen to intervenor arguments, and deals with license applicants at less than an arm's length" (p. 168).

This book, then, is a worthwhile contribution to the literature, at least as far as it goes. However, from the perspective of many readers of this *Review*, its strength is also its weakness. Although the author is aware of the political implications of this subject, much of the book reads like a legal brief replete with descriptive marginalia and conclusory statements. It is not until page 97 of the 169 pages of text material that the dominant social issues in the debate (e.g., the need for power) are spelled out. Civil disobedience, which transformed Seabrook from just another contested licensing case to a national symbol of the future of nuclear power, receives just four pages of discussion as an afterthought at the end of the book. A recent example of a much more satisfactory analysis of the conflict of political actors, their values, and the use of scientific evidence is found in Robert V. Bartlett, *The Reserve Mining Controversy* (Indiana, 1980). Nonetheless, Stever has presented an instructive, if limited, critique of the competence of the individuals and organizations charged with the responsibility of regulating the nuclear industry.

JAMES S. BOWMAN

University of Wyoming

Twigs for an Eagle's Nest: Government and the Arts, 1965-1978. By Michael Straight. (Berkeley, Calif.: Devon Press, 1979. Pp. 181. $5.95, paper.)

Michael Straight is a new type of government official: the cultural bureaucrat. As deputy chairman of the National Endowment for the Arts from 1969 to 1978, he helped guide that agency through a period of growth that has made it one of the great bureaucratic success stories of the past decade. But the very success of the Arts Endowment has emerged as a political issue. The advocates of public culture were once anxious to convince elected officials that supporting the arts was not an entirely frivolous use of the taxpayers' money. Now cultural bureaucrats are often at pains to assure the arts community that public funding is not a threat to aesthetic values or artistic autonomy. The Endowment has had to defend itself from charges that its effort to democratize culture is, in fact, a policy of cultural populism that would abolish aesthetic standards and artistic excellence. That some of these attacks come from within what could reasonably be considered the "cultural constituency" is particularly disturbing for the Arts Endowment.

Twigs for an Eagle's Nest is a book of personal reminiscences. While largely anecdotal, it does offer the kind of participant perspective that can provide invaluable insights into the policy-making process. As the second-in-command under the very aggressive and visible Nancy Hanks, Michael Straight was perhaps ideally situated to write a bureaucratic memoir. We also learn about the issues of cultural policy making with which the Endowment was faced in those formative years. First, there was the great fear that some Endowment-sponsored project would scandalize the public—or worse, a member of Congress. Second, there was a more positive belief in the principle of "cultural egalitarianism"—that "no church, no aristocracy, no elite of any kind was able to impose its own aesthetic standards upon the nation" (p. 54). Third, there was a commitment to government "non-intervention" in culture through the use of advisory panels—presumably composed of disinterested experts on artistic excellence.

Each of the issues indicated above were then, and continue to be, highly debatable. One can be so fearful of possible adverse public and/or congressional reaction to government-funded cultural projects as to bring about a form of self-censorship. Such a censorship can discourage the Endowment from sponsoring work that might be judged hostile to prevailing orthodoxies—aesthetic or political. Also, cultural egalitarianism can be the foundation for a broad-gauged and comprehensive policy of public culture. It can also be a justification for funding artistically mediocre if politically attractive grass-roots organizations at the expense of established standard-setting cultural organizations. Last, Straight's case for cultural non-intervention rests on a too-uncritical reliance on the panel system without analyzing the potential for bias inherent in panel members and their criteria for judgment.

But, as befits a memoir, Straight's look is longer on recollection than analysis. For the increasing number of political scientists who study cultural policy making, *Twigs for an Eagle's Nest* is invaluable as one of the only accounts written by a top NEA official. It is a good complement to Dick Netzer's economic analysis of public culture in *The Subsidized Muse* (Cambridge, 1979). For students of public policy making in general, Straight provides a good "insider's" perspective on managing a successful but controversy-prone public agency.

KEVIN V. MULCAHY

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The Zero-Sum Society: Distribution and the Possibilities for Economic Change. By Lester C. Thurow. (New York: Basic Books, 1980. Pp. v + 230. $12.95.)

American society has been paralyzed by the responsiveness of government to securitarian demands. "As we deliver economic security, we undercut the implicit assumptions of capitalism, democracy, and individual initiative" (p. 24). Solutions exist for all our problems—energy, inflation, over-regulation, declining productivity and growth. But every solution imposes a cost on somebody, and all solutions face organized or potential enemies. Deregulation of energy prices would unjustly redistribute income. Unwilling or unable to meet foreign competition, both managers and workers go protectionist, as in the case of steel and autos. No general will, no cumulative consensus, any longer exists to forge policy in the public interest. Nor is our political system able to deal with the paralyzing vested interests. Hence frozen pluralism is the essence of Thurow's diagnosis of the American malaise. His prescriptions for our condition command attention, for they offer a unique amalgam of competitive and egalitarian policies.

"If we are to establish a competitive economy within a framework of international trade and international competition, it is time to recognize that the techniques of the nineteenth century are not applicable in getting ready for the twenty-first century" (p. 145). Anti-trust actions are far less effective than exposure to foreign competition as the way to enhanced efficiency and welfare. Thurow advocates that we rely on market forces as far as possible. "Basically, price controls should be avoided whenever society is addressing any long-range problems" (p. 140). He is quite willing to deregulate the energy industry so long as we compensate those whose incomes would otherwise decrease severely. Indeed, he advances as a general principle that we compensate those individuals dislocated by economic progress, but not firms that should go under. And he recommends that compensatory programs be generous, even overly generous, anything to break the ice of securitarian paralysis. He further proposes that we get rid of the corporate income tax and establish a rational and fair tax system. Rather more controversial is his notion that we need the national equivalent of a corporate investment committee to help redirect investment from "sunset" to "sunrise" industries. Here he seems to be influenced by what may well be questionable interpretations of the dynamism of "Japan, Inc."

When Thurow is thinking competitively, he comes through as thoroughly un-tenderminded. "Man is an acquisitive animal whose wants cannot be satiated. This is not a matter of advertising and conditioning, but a basic fact of existence" (p. 120). He has nothing but contempt for those who think we could live with a zero-growth economy. Above all, "We need to start over and see what a modern economy needs in order to remain a competitive economy" (p. 150).

The other Thurow proposes to galvanize the economy by an injection of egalitarian justice. We must achieve a common conception of justice if we are to say upon whom and to what extent losses should be inflicted for the sake of the common good. And for him economic justice means more equality. Income transfer is not the method by which this should be brought about. Rather, "The basic problem is to change the structure of the economy so that the entire economy generates the kinds of jobs that are now open to white males and ensures that there are enough of these job opportunities to go around" (p. 203). This requires creation of a "socialized sector" that would operate on the basis of the earnings and differentials, a five-to-one ratio of average earnings of highest-paid quintile to average earnings of lowest-paid quintile that obtain in the "primary economy," now the preserve of white, fully employed males.

As I understand the reasoning behind this dramatic proposal, it goes as follows, not all of which Thurow makes fully explicit. In his *Generating Inequality: Mechanisms of Distribution in the U.S. Economy* (Basic Books, 1975), Thurow explained how "instantaneous fortunes" are created by disequilibria in the capital markets. They are really "prizes" in an economic "lottery." He now refers to our whole economy as a "natural lottery" (pp. 200-01), a game to which white males have privileged access. Now if indeed we are running a lottery, then we should give everybody a chance by starting up another in a "socialized sector." This idea that the economy is essentially a game of chance would seem to account for Thurow's rather odd understanding of "equal opportunity." "Presumably this phrase means that each individual should have an equal chance for economic success" (p. 195). Ordinarily, equality of opportunity refers to fair competition among those equally endowed and motivated, not to a game of chance in which all participate.

Given that Thurow's "socialized sector" is not a Civilian Conservation Corps, what exactly would go on in it? He speaks of "projects" and mentions two by way of illustration: street-cleaning projects in competition with municipalities and road-bed rebuilding in competition with private railroads. Such work is to be paid at the rates that obtain in the "primary economy." This all makes sense only if the connection between pay and performance may be broken with impunity or if the connection is at best tenuous, perhaps even nonexistent. Thurow's "lottery" conception of the economy suggests that the latter is his fundamental conviction. If he is right, then of course our economy is far less a domain of rational activity than it has been portrayed by most economists, for example, Sir Henry Phelps Brown in *The Inequality of Pay* (Oxford, 1977).

Has Thurow produced a coherent theory of political economy? No doubt many will find appealing his grand strategy that egalitarianism is the key to economic dynamism. But on Thurow's own terms, does the world really work like that? On the one hand, he sees us as economic men, and he is all for competition. On the other, he asserts that we are not highly sensitive to financial incentives, willing to work hard at differentials of five to one, and this would seem to imply that we are natural egalitarians. He does not attend to recent Swedish and British experience suggesting that equalization both reduces and misallocates effort. Nor is it easy to see how Thurow's economic men could reach agreement on a substantive conception of justice. Even John Rawls shys off from that, leaving economic outcomes to be shaped mainly by just institutions and procedures.

Nevertheless, Thurow has made an imaginative contribution to the great debate over principles of political economy and justice. He agrees with Hayek that the defensively organized stand in the path of economic progress, and they both would foster competition. But Hayek thinks social justice is a mirage. To promote both efficiency and equality, Phelps Brown would improve people's ability to work; the connection between pay and performance is both rational and just. In *The Structure and Reform of Direct Taxation* (Allen & Unwin, 1978), J. E. Meade would eliminate high marginal rates of taxation at both ends of the income scale to reinvigorate the economy. All of these economists think that rationality rather than luck governs economic activity. This is the fundamental postulate that Thurow challenges.

JOHN W. CHAPMAN

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How to Limit Government Spending. By Aaron Wildavsky. (Berkeley: University of California Press, 1980. Pp. ix + 197. $8.95.)

Aaron Wildavsky's argument is straightforward: growth of government, particularly at the expense of the private sector, is not desirable. He argues that controlling the expansion of federal spending will reduce inflation, lower taxes, and enhance efficiency. An absolute limit on spending or a balanced budget is inferior to keeping the percentage growth of federal outlays to the percentage growth of the GNP. Implementation of this goal cannot be achieved by administrative regulation or statute. Instead, only a constitutional amendment is sufficiently powerful to compel all governmental or quasi-governmental actors to submit to the same discipline. Resource addition, the name of the present spending game, will be replaced by real resource allocation if a constitutional outlay limit is enacted.

This book exemplifies two facets of Wildavsky's approach to policy analysis. Naturally, there is the careful analysis of the policy being investigated: craftsmanship. But there is also salesmanship. As he says in his *Speaking Truth to Power,* "My personal preference . . . is to supplement knowledge with persuasion by actively helping policy ideas make their way in this world" (p. 10). By keeping that orientation in mind and realizing that *How to Limit Government Spending* is based on the 1979 Fels Lectures at the University of Pennsylvania, one can understand the polemic and almost conversational nature of the book. The witty, breezy, lively, forceful, and argumentative flavor dominates the analysis as well as the presentation. This does not mean the analysis is shoddy. It is good but not overwhelming. I am heavily swayed but not totally persuaded.

One of the major difficulties in analyzing proposed policies that are largely without precedent is that certitude about cause and effect is lacking. The analyst must go to analogs or relatives of the policy. Since the experiences in the states are the closest we have to the real thing at the national level, their success should weigh heavily in the analyst's mind. It is a tribute to Wildavsky's intellectual integrity that an excellent appendix by Naomi Caiden on problems in implementing expenditure limitations in the states is included. As I read it, this careful appendix is properly not nearly as optimistic about the practical matters of implementation and eventual success as Wildavsky hopes. The analogous cases make me wonder whether the proposal will proceed as the policy advocates suggest.

Wildavsky is less than forthcoming on just how the analysis will be carried out in the government as agencies compete for the marginal increases available to the government or fend off redistribution of the money already allocated to them in years past. Since policy analysis is often closer to an art than a science, it is unclear just how the analysis would be performed and used in the halls and offices of government. Wildavsky does predict that conflict within government will increase. But it is uncertain just how that will affect the delivery of services. More time and other resources may be devoted to competition within government than to actual production. His analysis is structural and macro-level. It should cover more at the micro-level.

Overall, though, this is a very provocative examination of a policy proposal that will be receiving more attention in the near future. It is well written and presents a reasonable analysis. It is worth reading. On the level of salesmanship and polemic, it merits 4½ stars. On the level of analysis, I'd give it 3 stars.

JOHN WANAT

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The Politics of Regulation. Edited by James Q. Wilson. (New York: Basic Books, 1980. Pp. xii + 468. $18.95.)

According to James Q. Wilson and the authors of the nine empirical studies of regulatory enterprises included in this volume, there is a politics of regulation. It is a politics quite unlike that based essentially upon the maximizing impulses of industries and officials featured in theories propounded by economists. It is also unlike that vision of regulatory politics most familiar to political scientists and associated with concepts such as capture, life cycles, iron triangles, and interest group liberalism. For somewhat different reasons, both of these perspectives assert that regulatory behavior is fundamentally a reflection of economic power. At least during the past decade, many developments in government regulation have been inexplicable in terms of either of them. Although this book is not the first effort to grapple with disparities between conventional theories and regulatory behavior and to examine critically the fundamental assumptions of those theories, it is an especially ambitious and successful one. The view of regulatory politics that results is of major significance and utility for students of regulation and of American politics in general.

The analyses upon which that view is developed in a concluding chapter by Wilson are of several varieties of government regulation. Although not written to formula, they are compatible in format and emphases, and most importantly in their common interest "in how goals were determined, conflict resolved or managed, standards set, and policy enforced" (p. xi).

In addition to supplying raw material for the final essay, the individual studies are well executed and instructive in their own right. Douglas D. Anderson and Bradley Behrman, in their chapters on state utility rate regulation and the Civil Aeronautics Board, assess changes in established regulatory systems that conventional theories would not have predicted. In contrast, Edward Mansfield depicts the Federal Maritime Commission as a regulatory system that does not work and in which the regulated call the tune. The analyses of the Antitrust Division of the Department of Justice and the Federal Trade Commission by Suzanne Weaver and Robert A. Katzman—taken from their books on the topics—dissect the complex factors, especially professional norms, that shape case selection decisions.

Paul J. Quirk's study of the Food and Drug Administration attacks a fascinating paradox: why drug regulation simultaneously results in "inadequate protection for the consumer" and "excessive burdens" on drug developers. In regard to the Occupational Safety and Health Administration, Steven Kelman provides a comprehensive overview of the policy dilemmas faced by the agency in implementing a regulatory program strongly resisted by its targets. He shows how the precepts of the occupational health and safety fraternity have produced an aggressive regulatory program in the face of substantial opposition. Alfred Marcus uses the case of the Environmental Protection Agency to weigh the effects of precise legislative delegations of authority, concluding that detailed specification, despite the advantages claimed for it, may impede the attainment of policy goals. Jeremy Rabkin dissects another interesting paradox, this one presented by the Office for Civil Rights of the Department of Health, Education and Welfare. The concurrence of what many consider to be overzealousness in the production of rules and regulations pertaining to the treatment of racial minorities, women and the handicapped and a woeful record of effort in enforcement is traced to the mystique of civil rights which limits possibilities for bargaining.

Altogether, the studies reveal disparate patterns which must be accounted for in the origins of regulation, in agency behavior, and in the dynamics of regulatory systems. They show clearly the naive nature of most political theories of regulation and the deficiencies of economic theories. Wilson argues that the microeconomic theory now so prominent in the study of regulation, though not without value, is lacking, because politics differs from economics in important respects. Consequential elements in the regulatory process not encompassed in economic perspectives, but shown by the cases to be of critical explanatory importance, include policy preferences not readily expressed in monetary terms, heterogeneous coalitions which drive much political action, and the continuing efforts to change preferences which are at the heart of the political process.

The analysis of regulation, then, needs to be set within a framework which takes into account the political, and a framework sophisticated enough to allow for observable complexity, variation, and change. This Wilson supplies, relying upon the case studies for suggestions and illustrations of components and relationships. Emphasis is placed on processes of coalition formation and maintenance keyed to the distribution of policy costs and benefits, the balance within agencies among the norms of careerists, politicians, and professionals, and the regulatory environment, composed of such elements as technology, price structure, the political temper of the times and associated influential ideas about policies and institutions, and the dispositions of controllers, such as legislatures, executives and the courts.

The foregoing has not done justice to the particular aspects of the framework or to the stimulative character of the book and the wealth of insights it provides. Of these, one of the most important and interesting is the assertion of the importance of ideas about policies and institutions in setting the course of regulation, independent in their effects of "interests." But the book is not in all its parts beyond criticism. Some may find fault with aspects of the case studies and of Wilson's interpretation and application of their findings. Such quibbles, however, do not detract from the essential value of the book. It persuasively invalidates much conventional wisdom about regulation and proffers an attractive and plausible alternative route to explaining regulatory phenomena. That is an admirable achievement.

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Comparative Politics

Mothers At Work: Public Policies in the United States, Sweden, and China. By Carolyn Teich Adams and Kathryn Teich Winston. (New York: Longman, 1980. Pp. vi + 312. $9.95, paper.)

Despite two decades of ever-increasing female employment, political scientists have virtually ignored questions of public policy as they affect women who work and their families. In *Mothers At Work* Carolyn Adams and Kathryn Winston analyze why government policies toward mothers who work differ in the United States, Sweden, and China. Their treatment is straightforward, sensible, and devoid of polemics.

The authors have wisely assumed that the average reader is either poorly informed or misinformed with respect to social policy for working families. The initial portion of this book, therefore, offers an inventory of government programs in family planning, maternity benefits, child care, home helps and welfare assistance to single-parent families in the three countries. The authors make clear that in the United States—at the state and federal level—social policy makes scant provision for working women. With the exception of welfare mothers, the woman who works is encouraged by those who make policy to rely upon private markets rather than the public sector for medical services, housekeeping help, and child care.

In contrast, although delivery systems and central government involvement vary considerably between the two, Sweden and China provide extensive services for working mothers. The national government in Sweden absorbs most of the cost of its well-known and extensive programs. Under its unitary form of government, provincial and municipal governments must yield ultimate control of programs to the central government. China, by contrast, has relied upon local initiative and funding for the provision of services despite the highly centralized nature of its government. As a result, in China "local service delivery has been susceptible to frequent disruption because it has relied so heavily on voluntary contributions of time, space and equipment" (p. 26). Sweden and China, the authors conclude, while not always successful in actually providing all the desired, or even necessary, services, are at least committed in principle. National policy makers in the United States even hesitate to make a commitment in principle.

Students of family policy will appreciate that Adams and Winston are not content to describe comparative policies and to establish that significant differences exist. At the outset of their study, the authors indicate that they are "concerned with explaining the presence or absence in our three nations of a particular set of social programs to facilitate working women's dual roles as earners and homemakers" (p. 15). Analysis of this kind has been long overdue in family policy research. Presumably, it has not been done because of the differing disciplinary preoccupations of the historians, sociologists, and economists who have made substantial contributions to our knowledge of the relation of the family to the state.

In chapters 3, 4 and 5, therefore, Adams and Winston weigh whether input variables—interest group and party activity—or social and economic factors of the environment account for differences found in U.S., Swedish, and Chinese policy. They first examine whether women's own efforts to promote beneficial government policy have been influential. Swedish and Chinese women are described as social feminists whose demands focus upon improved living conditions for working women even where this requires *unequal* treatment. Women activists in both countries lobby from within large, usually male-dominated organizations—generally political parties or unions. Both this integrationist organizational approach and the nature of their demands differ from those of American women who operate out of mass-based, separatist groups almost exclusively advocating equal rights.

The significance of these differences in women's political action becomes unclear, however, because the authors conclude that policy outcomes are more dependent upon the labor market and attitudes concerning the relation of the state to the family. Adams and Winston are not decisive about which of these factors exerts the stronger influence on policy initiation and implementation. Perhaps their waffling reflects the wisdom of those who know when precise conclusions are premature. *Mothers At Work* reviews a considerable amount of policy in three very different societies. The study opts for breadth rather than depth. Yet depth is required for precise analysis. I would argue, for example, that they underestimate the impact of women activists in the development of child care policy in the United States. By defining most U.S. women's political activity as equal-rights focused, they ignore the role of social feminists like Joyce Miller of the Coalition of Labor Union Women, Marion Wright Edelman of the Children's Defense Fund and, earlier, Elinor Guggenheimer and others. This leads them to deemphasize the impact of mass politics on the development of policy for working women. China specialists may argue that the authors have artificially fit that society's single-party state into an interest-group model and underplayed the debate between economic managers and revolutionary party cadres.

Despite such reservations, *Mothers At Work* serves its audience well by being broadly comparative and, in the best tradition of intellectual inquiry, generates many new questions. It presents analysis where only description existed before. Those of us who work and teach in this field must thank the authors for this important first step.

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The Civic Culture Revisited: An Analytic Study. Edited by Gabriel A. Almond and Sidney Verba. (Boston: Little, Brown, 1980. Pp. viii + 421. No price given, paper.)

"It was the particular nature of the approach of *The Civic Culture* that made it an inspiration to some to do the same elsewhere, to others to do something similar but better, and yet others to do something different" (p. 396), writes the junior author of *The Civic Culture* (Princeton, 1963). This uncharacteristically unverifiable generalization (uncharacteristic for Sidney Verba) appears in a "personal postscript" to a volume which promises "to revisit the civic culture, to subject its philosophy, its methodology, its national and comparative findings to searching critiques by appropriate scholars" (p. v). The criteria for choosing "appropriate scholars" are not presented—only one contributor, Carole Pateman, is well known as a critic of the study—nor are most of the critiques particularly "searching." It also seems curious for Almond and Verba to edit a critique of their own work, but to their credit they generally bend over backwards to acknowledge their shortcomings. Verba's modesty, apparent from the opening quotation, is reflected in word games—"foolhardy" is chosen in preference to "bold" or "incautious" to describe the decision "to apply a new method on an unprecedented cross-national scope to a problem never studied by that method" (pp. 394-95), and in some dithering over how to describe the book's impact on the study of comparative politics. Verba gives an all-too-brief account of the continuing difficulties of developing reliable cross-national generalizations, separating stable from labile attitudes, and "connecting survey data and macropolitical outcomes" (p. 404).

An essay by Gabriel Almond begins the book and provides an illuminating account of the Civic Culture study's intellectual antecedents. He describes but does not defend the derivation from political philosophy of the "rationality-activist model of democratic citizenship" (p. 16) and its incompatibility with stable democracy despite the fact that Carole Pateman devotes her essay to an attack on "its theoretical inadequacy" and "celebration of the role of political apathy and disinterest" (p. 58). Without mentioning her specifically, Verba indirectly acknowledges Pateman's complaints about a "rather one-sided approach to stratification" (p. 407), but defends the choice as empirically sound and justified in terms of their dependent variable—democratic stability.

Comments on the methodological and philosophical underpinnings of the study are provided by Arend Lijphart, Carole Pateman, and Jerzy Wiatr followed by essays on each of the original five nations. The latter are written to a formula to include a discussion of the impact of *The Civic Culture* on scholarship within the country concerned, shortcomings in that country's original treatment, and changes since then in the portrait drawn by Almond and Verba, as well as exploration of aspects of the country's political culture not covered because of sins of omission, methodology, or changed circumstances.

Lijphart is quite gentle and in fact enters the lists on behalf of Almond and Verba to battle critics such as Brian Barry (on the operationalization of democratic political structure) and Erwin Scheuch (on the individualistic fallacy). He does criticize Almond's and Verba's failure to anticipate the consociational route to stability in antagonistic mass level circumstances and a "line of reasoning [which] links an inadequate civic culture to the poor quality of democracy instead of to democratic stability" (p. 52).

Carole Pateman subjects *The Civic Culture* to a careful scrutiny on philosophical and logical grounds and finds it wanting in a variety of respects, most of which she traces to the blinders provided by a liberal pluralist notion of democracy. Craig and Cornelius, in their chapter on Mexico, accept one of Pateman's major empirical criticisms—of the assumption that efficacious replies to questions on citizen and subject competence "accurately reflect system responsiveness" rather than "the respondent's estimates" (p. 357) or acceptance of system norms. Other points are echoed by Lijphart and Wiatr. She is otherwise ignored, despite the fact that her analysis strikes at the heart of the study as a contribution to empirical democratic theory.

The country chapters are impossible to summarize adequately. They present valuable reviews of the literature on political culture and related topics for each country, in addition to the retrospective evaluations already mentioned. In his chapter on Germany, David Conradt comes closest to a true replication by comparing changes over time in two or three of the five countries. He argues that Almond's and Verba's pessimism about that country's democratic prospect is no longer warranted. Giacomo Sani does not dispute their principal generalizations regarding Italy, and believes that, if anything, negative evaluations of the system are even more pronounced now despite (or because of) the erosion of the Catholic tradition and the legitimation of the Communist party. However he does point out that "reticence," a feature of Italian political culture which Almond and Verba noted may have affected the quality of responses, and led to an underestimate of political involvement.

Dennis Kavanagh and Alan Abramowitz take responsibility for tarnishing *The Civic Culture*'s optimistic image of the United Kingdom and the United States, respectively. Both see levels of trust as having declined although Kavanagh sees the change as general and permanent and Abramowitz as specific (related to particular issues) and temporary given "apathy, the reluctance of political elites to mobilize support for structural reforms and the ability of the two-party system to defuse and channel discontent" (p. 205). The implications of these different perspectives for the book's theory of democratic stability are not followed up.

The chapter on Mexico notes the problems for comparison and interpretation presented by a sample which ignored rural areas, translation errors, and "problems of validity or equivalence" (p. 326). More than the other authors, Craig and Cornelius are forced to correct the Almond and Verba portrayal of "their" country, especially on the measurement of system responsiveness. The concept of efficacy or competence is judged useful but better applied to the policy implementation stage and an analysis of patron-client relationships, rather than to respondent impact on "rule making."

How well does *The Civic Culture* stand up to empirical scrutiny? While taking issue with particular aspects of the study, with the exception of Craig and Cornelius, none of the country authors challenges the essential features of the descriptions originally provided. Changes are detected in the political cultures of Germany, the United States, and the United Kingdom which do not conform to Almond and Verba's expectations, but efficacy, trust, and involvement (with the significant addition of unconventional forms of participation) remain the dimensions whose measurement is attempted. All bemoan the neglect of intra-nation variation noted by Stein Rokkan in his original review (*APSR,* Vol. 58 [1974]) and would agree with Verba that the focus on system survival "did somewhat reduce the fruitfulness of the work" (p. 407).

Paradoxically, given its target, the book is insufficiently comparative. It is left to the reader to speculate about the inter-nation generalizations so prominent in *The Civic Culture,* and to synthesize a comprehensive methodological and theoretical assessment.

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Die Besiegten von 1945: Versuch einer Politologie für Deutsche samt Würdigung der Politikwissenschaft in der Bundesrepublik Deutschland. By Hans-Joachim Arndt. (Berlin: Duncker & Humblot, 1978. Pp. 462. DM 98.00, paper.)

Hans-Joachim Arndt's book performs a serious disservice to political science—not to speak of the damage it inflicts on the West German political science profession. At a time when scholars are trying to break their chains of parochialism, focus their attention on fundamental aspects of politics across time and space, and develop modes of communication among those who hold different perspectives, he chastizes German political science for failing to do precisely the opposite.

The catchy title and first subtitle—"The Conquered of 1945: Toward a Political Science for Germans"—tells the story of the first quarter of the book. Twenty-two years old when the Nazi régime collapsed in 1945, Arndt studied at a time when most West German intellectuals were trying to come to terms with their recent history, rebuild a profession devastated by Hitler's politics, and catch up with the thinking and methodologies developed by their colleagues (and frequently former compatriates) in other countries, especially the United States. Yet, ultimately, he argues, they failed to grasp a central "fact": any science of politics must begin with and proceed from the concrete basis of politics in a particular place at a particular time. For postwar Germans, he continues, that "concrete basis of political science" was their country's defeat in 1945. Their failure to understand this has led them to produce the political science of a conquered people, not of Germans.

Strong words, those! We may ignore the arrogance of such a view—and the possibility that some Germans may have seen as more central issues their nation's earlier willingness to embrace Nazism, the problems brought on by the division of Germany, or even the prevention of nuclear war. More problematic in my view are the intellectual and practical implications of a nationalistic political science, one so oriented to one's own country that Arndt can brusquely dismiss any search for regularities across systems and across time, and, for the most part, the intellectual legitimacy or even relevance of such subdisciplinary concerns as electoral behavior, international relations, and most aspects of political theory.

The remaining 362 pages, a tenth of which is bibliography, detail the postwar history of the West German political science profession. Arndt classifies full professors according to whether they tend toward historical-descriptive analysis, bourgeois democracy, social-liberal reform, systemic change, or sympathy toward communist groups, and according to the universities in which they were trained and where they found positions. He classifies articles in three major journals in a variety of ways, including the four general tendencies toward the constitutional state, "scientism," class society, and historical-descriptive analysis. And he describes the development of individual scholars and their "schools," stopping now and then to present case studies of critical turning points in the profession, such as the German Political Science Association's annual meetings of 1973 and 1975.

The description itself is not without interest—*provided* that the reader is sufficiently aware of the circumstances being described to realize that the account is a highly personal one with which doubtless no other German political scientist would agree. (That anyone would discuss in such loving detail the recent history of the American political science profession, including the stormy business meetings at the APSA annual meetings of 1968 and 1978, boggles the mind!) It is interesting to learn, for instance, that people trained in states governed by Social Democrats seem to end up with jobs in Social Democratic states, and that those trained in states governed by the Christian Union parties find jobs in the same states. We are similarly interested but not surprised to learn that the founding fathers of postwar German political science and their earliest students were by and large classical democratic theorists, whereas the younger generation, those born since 1934, are more critical, with the bulk of them even favoring changes in the system.

But ultimately it is precisely this personal characteristic that defeats Arndt's account. His classifying schema, insofar as they are explicated, seem designed to produce a particular result. Much of the descriptive portion seems to carry over into print arguments the author had with his professional colleagues. Moreover, as noted earlier, by writing off as derivative of American political science whole fields such as electoral behavior and international relations, Arndt's general position loses credibility. When Arndt concludes *inter alia* that it was the absence of a "basis in concrete reality" which led the German political science profession to orient itself first toward the approach of the American conquerors and then, after 1966, toward that of the Soviet conquerors, the reader—this one at least—remains unconvinced.

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China's Four Modernizations: The New Technological Revolution. Edited by Richard Baum. (Boulder, Colo.: Westview Press, 1980. Pp. xx + 307. $18.75, cloth; $9.50, paper.)

Not the least of the remarkable events in China since the demise of Mao Zedong in 1976 has been the leadership's outspoken commitment to a comprehensive and ambitious program of economic development. Incorporated into the original, and now revised, 10-year plan, and serving as the foundation for goals to be achieved by century's end, the "four modernizations" (agriculture, industry, science and technology, national defense) has become a theme as prominent and deeply inculcated in the China of today as "politics in command" was a generation ago.

Originally prepared for a gathering of Sinologists in early 1979, the papers contained in this volume probe a variety of aspects of this effort, explaining its origins and present status, analyzing the difficulties which confront China in attaining the stated goals, and assessing the likelihood of the scheme's realization. Appended to each piece are abbreviated "comments" by other participants in the conference.

Authors and commentators on China's current development campaign agree on three points: (1) scientific and technological advances are imperative to the country's overall economic modernization, and underpin the success of the three related "modernizations"; (2) there are serious, perhaps insurmountable, difficulties confronting the Chinese leadership in achieving even modest success in their inordinately ambitious program; (3) less obviously, but perhaps more significantly, the political climate that has most recently been favorable to "modernization" cannot be assured indefinitely. Each of the authors considers at least one of these points and several, including Richard Baum in his introductory essay, note the connections among them. In sum, the pieces offer the most explicit and thoughtful treatment of China's current economic drive available to Western readers.

Even the seasoned reader on domestic Chinese politics is struck by the tenor of these articles: the appallingly backward state of Chinese science and technology and the scale and variety of the society's needs on this score, in spite of some significant gains in the past decade. If the leadership is to succeed in expanding and refining the economy, especially the industrial sector, there is no choice but to expend all reasonable effort in acquiring, absorbing and disseminating—as rapidly and comprehensively as possible—substantive scientific and technological knowledge, skills and capability. The conference participants all agree that, short of this, there is little hope that China will be able to "catch up" with the industrialized superpowers, a goal the Chinese themselves consider critical to the country's national security.

Yet, as several of the authors explain, the problems are immense. "Science" has traditionally been a bit of a bogey in Chinese culture, a vestige of pre-modern China that lingers on in some quarters. More immediate are the obvious and practical problems of how the People's Republic can possibly integrate into the system the degree and extent of contemporary technology necessary to this objective, especially given the lack of a broad-based, complementary scientific and social infrastructure.

Politically, the problem is manifold, and in the long run, potentially the most troublesome. All of the authors in some way refer to the contrary ideological line promoted, and to a large extent implemented, by the now-purged Gang of Four in the period preceding Mao's death. Those unfamiliar with the vagaries of domestic Chinese politics would be prudent to consider carefully the consensus of most of the essays in this volume: that numerous chapters in China's twentieth-century political history remain to be written, and there is no reason to assume that just because radical politics is in eclipse currently, it will necessarily remain so indefinitely.

The *ti-yung* (essence-method) controversy prominent more than a century ago in China seems to be as pertinent as ever: can the "methods" of Western science and technology be introduced and integrated into Chinese society without detrimentally affecting the "essence" of Chinese values? Unlike the values at stake in the nineteenth century, however, those in question currently are the core of revolutionary Maoist ideology.

As many of the authors note, the implications and consequences of contemporary science and technology, which China must somehow acquire from the industrialized and capitalist West, cannot be limited solely to the laboratory or the work place. The likely political impact on Maoist values and norms is unclear. It might be, as Baum notes in his introduction, that the Gang of Four was correct in its assertion that Maoist ideological purity and commitment would fade as modernization (viz., Chinese economic development based on rational scientific and technological knowledge as pioneered in the industrialized West) proceeded.

One cannot also avoid wondering whether Mao's admonition "never to forget class struggle" might not again serve as the basis for a radical swing of the domestic political pendulum. Radical swings are not uncommon in China; it is a leveling thought to consider the possibility and potential consequences of such an event in China's future.

KENT MORRISON

University of Utah

The Indian Supreme Court and Politics. By Upendra Baxi. (Lucknow, India: Eastern Book Company, 1980. Pp. xvii + 272. Rupees 75.)

Public law specialists are given to describing the American Supreme Court as "a uniquely powerful political institution" (Funston, 1978). And in *Gideon's Trumpet* (1964), Anthony Lewis proclaimed the Supreme Court as "different from all other courts, past and present." If taken literally, one must conclude that the American Court is sui generis—constituting a class of one, precluding comparison with constitutional courts in other polities. Without underestimating the important role played by the Court in American politics, we ought to be able to acknowledge the presence of politically significant judicial institutions elsewhere, if indeed such exist.

For the 31 years of its existence, the Supreme Court of India has been an enormously important factor in Indian politics. The Court has declared hundreds of national and state laws unconstitutional and, since 1967, has even *successfully* claimed the power to invalidate constitutional amendments, most of which were passed to reduce the Court's jurisdiction and powers. India's first midterm election was precipitated by two Court decisions, and, in June 1975, a few hours after the Court made a preliminary ruling with the result that a lower court judgment finding Prime Minister Indira Gandhi guilty of corrupt election practices remained in effect, Ms. Gandhi proclaimed the Emergency that was to last nearly two years. Modeled on the American Court but functioning within the Westminster model of parliamentary democracy, the Indian Supreme Court, in terms of overall political significance, is probably the nearest analogue (not just the nearest non-Western analogue) to the United States' Supreme Court.

Although Indian scholars have regarded the Supreme Court as an important institution, Baxi's book is the first to view it as an important political institution, by which he means that the Court exercises significant political power, and that the judicial process "is a species of political process and that constitutional adjudication is essentially a political activity" (p. 28). While this may be conventional wisdom about the American Court, there is nothing conventional about this thesis in India. Indians are acutely uncomfortable about implied or real linkages between politics and courts, and Baxi's book, by Indian standards, represents a shocking but welcome departure from conventional writings.

Upendra Baxi is a brilliant, young Bombay- and Berkeley-educated lawyer who teaches law at the University of Delhi. He is probably the most respected of the second generation of post-independence Indian legal scholars. Unlike the vast majority of Indian students of the Court, political scientists included, whose orientation is Blackstonian and whose analysis is characterized by uncommon vapidity, Baxi is perhaps closest in his orientation to the school of sociological jurisprudence, and he presents his analysis in a lively, exciting style.

Baxi clearly prefers that he and other Indians be governed by judges rather than today's politicians, and this book is an extraordinary brief for the exercise of judicial power, e.g., "The interests of the people require the Court to *lead* and the legislature to *follow*" (p. 248, italics in original). He seems to arrive at this position because of "the nagging political realities of India" (p. 184), by which he apparently means the absence of any significant political opposition in Parliament during all but two years of the past three decades, and the declining quality of political leaders. For Baxi, these political context factors and the social and cultural contexts as well, require the Court to function "as a parallel legislature and quite often as a parallel constituent body" (p. xii).

This book is addressed to an Indian audience, and assumes prior knowledge about the Court. Moreover, the potential reader ought to be alerted to the fact that Baxi's focus in on the decisions, and the environment of those decisions, of the 1975-1979 period, i.e., the period of the Emergency and the two years of non-Congress rule. Thus the non-specialist reader would benefit from having access to a companion volume which covers more ground, a good one being Rajeev Dhavan's *The Supreme Court of India* (Bombay: Tripathi, 1977).

Perhaps it is characteristic of books of this nature to be better at provoking than proving.

Somewhat annoying is Baxi's tendency to equivocate at critical junctures. He was deeply disappointed by Supreme Court decisions that effectively endorsed the grisly features of the Emergency, but he acknowledges that had the judges done much more, the Court probably would not have survived. He manages to castigate, applaud, and forgive the Court for the infamous Emergency habeas corpus (*Shiv Kant*) decision. Even his most original argument—that the post-Emergency Court has pursued an increasingly people-oriented "frankly populistic" (p. 247) posture, is searching for new bases of legitimation of its power, and seeks to speak to the people at large—though consuming half the book, is too untidy and loosely presented to be persuasive. And one must seriously question whether *any* constitutional court, in either institutional or personnel terms, is adequate to the tasks Baxi would wish the Indian Court to perform. But if the book is at times incautious, it is also a truly seminal study, the most pioneering and most important ever written about the Supreme Court of India.

GEORGE H. GADBOIS, JR.

University of Kentucky

Pakistan Under Bhutto, 1971-1977. By Shahid Javed Burki. (New York: St. Martin's Press, 1980. Pp. xi + 245. $22.50.)

Shahid Javed Burki undertakes to explain Bhutto's rise and fall in terms of conflicting class interests in Pakistan. Bhutto won the 1970 election because the urban and rural poor and the urban middle class supported him. By 1977 the latter had turned against him. His nationalization of private schools pulled standards down and alienated the urban middle class whose children attended, and whose women taught in these schools. The nationalization of "vegetable ghee" plants (despite earlier assurance that there would be no more nationalization) projected Bhutto as a covenant-breaker and scared many industrialists out of the country. The nationalization of several thousand flour and rice-husking mills needlessly alienated small business people. Bureaucratic reforms, by deemphasizing merit, hurt the middle class whose sons hoped to enter the higher civil service through competitive examinations. Bhutto's emphasis on a few grand but long-gestating projects starved many smaller development projects which would have expanded employment. Above all, the middle class began to suspect that Bhutto intended to disestablish Pakistan's parliamentary system to further augment his authoritarian rule. These points are all well made.

Most observers agree that Bhutto would have won a comfortable (though not an overwhelming) majority in 1977 even if the election had not been rigged. What then is the import of Burki's reasoning? He implies that Bhutto's reforms and authoritarian rule had so polarized the polity that his opponents would have waged mob violence against him, no matter how fair the election had been. His own supporters, the urban poor, stayed home because those who would have mobilized them—the PPP "left" led by Mubashir Hasan and Haneef Ramay—had deserted the prime minister.

It is not correct that the entire urban middle class had become hostile to Bhutto. Similarly, Burki exaggerates Mubashir's and Ramay's socialism and their hold on the urban poor. He would be on firmer ground if he were to focus on the PPP's general organizational debility for an explanation of the urban poor's passivity.

Burki does not deal with the fact, now freely acknowledged by PNA notables, that they and Bhutto had come to terms by the evening of July 4 with only a couple of minor points left to be settled the next day. Yet the generals struck later the same night. Nor does he refer to reports that while negotiations with Bhutto were in progress some PNA politicians, notably Asghar Khan, urged the generals to seize power. These reports may support the position that the polarization, referred to above, had made military intervention inevitable. But we do not know how plausible this argument is, since Burki does not choose to make it open and explicit.

Burki does not discuss any of Bhutto's positive contributions to Pakistani political culture, not even that he gave his nation's poor a measure of self-respect and awakened them to the importance of political participation. In the absence of any convincing factual basis and appropriate analytical framework, his reference to the allegedly low social origin of Bhutto's mother, as a clue to his authoritarian disposition, would seem to have no scholarly purpose or justification. How strange that the censors in Pakistan have nevertheless banned this book.

Despite the above reservations, this is an exciting book. Burki's initiative in studying social forces and their conflicts as major influencers of political development in Pakistan needs to be carried forward.

ANWAR H. SYED

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Which Way Is South Africa Going? By Gwendolen M. Carter. (Bloomington: Indiana University Press, 1980. Pp. xii + 162. $12.95.)

South Africa exhibits all the requisite conditions for revolution. Nonetheless, white dominance remains firmly entrenched. How much longer? How? Can peaceful, evolutionary change away from racially defined categories occur?

Gwendolen Carter, the most distinguished American analyst of the Republic of South Africa, takes no exception to the likelihood of basic transformation. "Change is in the air in South Africa," she opens (p. 1), consciously echoing former British Prime Minister Harold Macmillan over a score of years ago. By the end of the book, however, one is left—necessarily—with intelligent surmises rather than hard predictions, general tendencies rather than assured outcomes.

This volume draws on close to 30 years' detailed study of the Republic of South Africa. In 1958, Carter published *The Politics of Inequality* (Praeger), a detailed exigesis of apartheid legislation that remains the standard reference. In 1967, she coauthored *South Africa's Transkei: The Politics of Domestic Colonialism* (Northwestern), which sketched a gloomy prospect for what was to become the first "independent" "homeland." Between 1972 and 1977, she and three collaborators published four volumes of source materials, *From Protest to Challenge* (Hoover Institution), a useful compendium of documents from or about groups opposing minority control in South Africa. Carter's criticisms of government policies made her, in effect, persona non grata in the republic; for close to 15 years, her research was conducted from the distant reaches of Smith College, Northwestern and Indiana Universities, and other settings. Only in 1979 could she return for extended field work, and examine areas neglected in her prior publications, notably black consciousness.

Which Way Is South Africa Going? treats domestic rather than international politics, even though the external environment has changed far more than the internal. The buffers of the white redoubt have essentially disappeared since the 1974 Portuguese coup d'état. Now-independent Angola offers sanctuary to a liberation movement (SWAPO) the South African government detests; Mozambique has an avowedly Marxist government economically linked to South Africa through the port of Maputo; Zimbabwe has gained independence since the manuscript was completed; Namibia remains a bone of international dispute and guerrilla contention, the five-power initiative of 1977-78 apparently having bogged down. These are noted rather than discussed. Potential implications of a serious, effective international trade embargo remain unexplored (what *would* happen to the South African economy were its gold exports boycotted?), while the effects of long-term interruptions in petroleum shipments deserve speculation. What Carter says about the military potential of South Africa cannot be gainsaid, however: it boasts "by far the strongest and best-equipped" armed forces on the African continent (p. 114). Blacks are quietly being recruited and trained for combat against external incursions. A Security Council embargo against arms shipments has been effectively circumvented by domestic manufacturing. As a result, Carter reaches a conclusion with which some may quarrel: "South Africa's ability to defend itself against military action from outside seems assured. Its fundamental, and in the long-run possibly fatal, weaknesses lie inside its borders" (p. 126).

The interaction between Afrikaner politics and black politics thus holds the key to South Africa's future. The Afrikaners—proud descendants of the Dutch who wrested most of the Republic from the Africans—give scant indication of being willing to share power, even under extreme pressure. The blacks have been divided by historic antagonisms, dramatically sharpened by the "separate development" strategy of the government. Ultimately, one is forced to conclude that the present South African situation is hopeless, but not serious, terminal in its present state but buoyant at the moment. The eruption of black consciousness, the Soweto riots, and continuing external pressure notwithstanding, the Afrikaner government remains firmly entrenched, having further sharpened its legal and coercive instruments for excising dissidence. The winds of change, though more than zephyrs, remain far from hurricanes.

There are problems with the book, as the author herself best knows. Carter walks an academic tightrope 'twixt contemporary journalism and historical scholarship, 'twixt valued brevity and necessary detail. This volume, published in July 1980, includes references to events barely six months earlier, a record of updating few university presses can manage. Compression means that nuances are mentioned rather than explored; events are outlined instead of analyzed in depth; the recounting of facts is at times breathless rather than reflective. *Which Way Is South Africa Going?* will not answer its focal question to the satisfaction of all, but it gives the reader a clearer idea of the complexities of South African politics than any other book now on the market.

CLAUDE E. WELCH, JR.

State University of New York, Buffalo

Asian Women in Transition. Edited by Sylvia A. Chipp and Justin J. Green. (University Park: Pennsylvania State University Press, 1980. Pp. x + 262. $16.00, cloth; $8.95, paper.)

This edited book contains country-by-country chapters dealing with the role and status of women in Asia, both historically and at present. No methodology or rigid guidelines were imposed on the contributors, but most chapters include sections on general background, socio-cultural (especially religious) norms, and economic and political constraints. Some selections contain case studies while others include attitudinal surveys. A couple of the selections are updated versions of earlier work.

The thesis presented is that the role and status of women in Asia is not static, but in a state of flux. Change of all kinds in recent decades is affecting the position of women in a variety of ways. Some of the changes in Asia have had a positive effect on the situation of women, others have been more superficial than genuine, and still others have been challenged by a reactionary backlash. The book makes clear that the ultimate problem of women in Asia generally is that they have been, and still are, excluded from the mainstream of economic and political power. Indeed, in some of the countries examined, the concern must be focused at a much lower level of aspiration: the problem of women who are denied basic choices concerning the conduct of their own daily lives. The key analytical questions posed are (1) what are the economic, social, political, ideological, and religious factors affecting the process of role and status changes? and (2) will changes in women's roles really alter their status very much?

The selections in the book are well balanced between East, Southeast, and South Asia, and some clear area contrasts emerge. The position of women in Southeast Asia is considerably higher than women's position in either East or South Asia. This is apparently the result of the bilateral (rather than patriarchal) kinship system, and of traditional customs which accord the female high status. The position of women is highest in the Philippines and Thailand, and somewhat lower in the Muslim countries of Malaysia and Indonesia (with the exception of Java, where there is the potential for high status).

The position of women in East Asia is not as high, but the paradox there is that the real status of women is higher than the ideal. In the two chapters on China, it emerges that, while the Communists are ideologically committed to equality for women, whenever this goal conflicts with economic aims (for example with unemployment problems or child care costs), equity is shelved.

However, it is in South Asia that the status and role of women is the most depressing. Religious and cultural norms, Muslim and Hindu, place women at the bottom of the social ladder in a situation that offers virtually no mitigating factors.

This book would serve as a good text for women's studies courses and an interesting supplementary book for Asia courses of various disciplines. It fills a gap in the literature, and it is well presented. The selections on Indonesia, Malaysia, China, and the Muslim areas of South Asia are particularly enlightening. The main faults of the book are faults of omission. A number of interesting places are not covered, for instance, Singapore and Vietnam. Also, some of the contributions (e.g., on the Philippines and India) are too narrow in scope to provide an overall picture. Most disappointing is the limited treatment accorded to Hindu India. It consists of only one chapter which concentrates almost entirely on the political skills of Mrs. Gandhi. While Mrs. Gandhi is an interesting subject, the absence of additional, more general material on the position of Hindu women seriously short-changes a large and important area of consideration. Nevertheless, as it stands, the book is certainly to be recommended.

DIANE K. MAUZY

University of British Columbia

China's Global Role: An Analysis of Peking's National Power Capabilities in the Context of an Evolving International System. By John Franklin Copper. (Stanford, Calif.: Hoover Institution Press, 1980. Pp. xv + 181. $7.95, paper.)

Many observations have been made about China's status as a second-ranking power, but not so much work has been done to evaluate systematically China's power capabilities. John Copper's book fills this gap considerably. It is a welcome addition to Chinese studies.

Using numerous statistics and comparative figures, the author examines carefully several elements that determine China's power status. These elements include geography, population, natural resources, economic strength, military power, political system, and science and technology.

In the geography and population factors, Copper discusses the advantages and disadvantages of China's geographic situation and population size in comparison with several other nations. Although China's geographical condition is difficult for an enemy's invasion and occupation, her population size may not be "an asset" in germ, chemical, or conventional warfare on Chinese soil (pp. 22, 28).

Copper examines China's natural resources, ranging from basic grain production to up-to-date energy capabilities. One of China's important resources, uranium, which is essential to nuclear development for both peaceful use and military purposes, is not discussed. Uranium deposits in China are reportedly ranked seventh in the world, behind the U.S.A., the USSR, South Africa, Canada, Zaire, and Czechoslovakia.

Copper's statistics show clearly that, in terms of economic strength, China is still a backward country. In military power, the author finds that the number of military personnel (over 4 million, p. 75) and conventional weapons is of only relative significance. The lack of modern and sophisticated weapons, as all are aware, is the main weakness of the Chinese military power. But, if we take China's "defensive military policy" into consideration, we will see that China's second-ranking military power is also determined by its defense policy.

Copper discusses the well-organized and efficient Chinese political system but also asks whether the Chinese government is a totalitarian regime (p. 95). When we look at the turmoil during the Cultural Revolution that forced Beijing to recall all of its ambassadors, except one (Huang Hua in Egypt), for "consultation," we will have to acknowledge the fact that the Chinese government is still a centralized regime with not necessarily a stable leadership. Power struggles among factions are interpreted by the Chinese leaders as only the party and anti-party struggles, regardless of the fact that the previously purged "anti-party" faction has turned out to be the party leadership today.

Copper rightly points out China's backwardness in the field of science and technology when compared with other powers. Undoubtedly, political campaigns and ideology present a serious obstacle to progress in the field (p. 136). Finally, Copper concludes that China can only be regarded as a second-ranking power and that she will not become the dominant power in Asia (p. 150).

Two points can be raised here. First, the problem of overpopulation (pp. 23, 134) merits some elaboration. Recently, visiting scholars from China to the United States strongly indicate that overpopulation in China has entailed serious problems that certainly have slowed the economic growth. These problems include food, housing, employment, education, and even social unrest. They still appreciate economist Ma Yi-ch'u's "lost" warnings of overpopulation in the early 1950s. This dimension of evaluation can hardly be achieved by statistics.

Second, it is hard to predict that China in the future "will not become the dominant power in Asia." If the definition of "dominance" means the Soviet *domination of* the Eastern European countries, China will not obtain such a power status. But if it means the U.S. *influence over* the American states in the West Hemisphere, China may reach it in Asia in the future provided that China enjoys a few decades of peace. The magnitude of China's relative power can be more accurately measured at that time.

This book, nevertheless, is a fine and scholarly work. It is quite useful to students in the field.

KING C. CHEN

Rutgers University

Ideology, Public Opinion, and Welfare Policy: Attitudes Toward Taxes and Spending in Industrialized Societies. By Richard M. Coughlin. (Berkeley, Calif.: University of California Institute of International Studies, 1980. Pp. xvi + 195. $4.95, paper.)

This book presents a variety of survey data on public attitudes toward welfare policy for eight Western democratic nations—Australia, Canada, Denmark, France, Sweden, the United Kingdom, the United States, and West Germany. Survey responses are examined which pertain to broad ideological support for "welfare state" activities, for specific welfare programs, and for recent "tax revolt" initiatives. Unfortunately, the book offers little beyond a descriptive presentation of a rather mixed collection of survey data. Two principal shortcomings justify this conclusion.

First, the book suffers because it lacks a theoretical rationale. Coughlin offers neither hypotheses nor theoretical puzzles to be analyzed. Obviously, several theoretical postures are possible here and each would link the data to be examined to some particular explanatory focus. Yet Coughlin adopts none. He claims a concern with explaining "welfare state development," but he backs away from postulating any particular relationship between mass attitudes and welfare policy. And he has no data suitable for testing developmental hypotheses. He attempts to compare levels of public support for welfare policies with levels of program content. Yet, his conclusions about these comparisons are purely descriptive.

Ultimately, this absence of theory is the most important shortcoming of the book. Without a clearly conceived focus, the book breaks down into three descriptive presentations of survey data, themselves only weakly interrelated, with little effort to tie their results to any centralizing issues of more than topical interest.

Related to the absence of a theoretical focus is Coughlin's failure to take account of several bodies of research of notable relevance to his topic. One of these concerns the appropriate character, if any, of causal linkages between mass preferences and public policy. Research on this topic has, of course, been particularly extensive on the American case. A second body of literature is the more recent spate of "comparative policy" studies, many of which focus on some of the same issues of interest to Coughlin. A third body of research, largely focused again on the American case, is the controversy about whether there exist consistent ideological postures or, for that matter, specific policy-relevant attitudes at the mass level. The literature in all these areas is extensive, and each is of direct concern to Coughlin's work. Yet, none of the relevant literature is addressed in this study and the topics themselves are ignored or, in the case of the first one mentioned above, shrugged off as beyond the scope of the present study.

Second, the book is a severe disappointment methodologically. Coughlin offers an "analysis" of his various survey data which is, at best, a generation out of date for systematic social science. What passes for empirical analysis is the presentation of side-by-side frequency distributions for responses to somewhat similar (but sometimes rather different) survey items for eight nations at various times over the last 30 years. The data are at points very problematic as to comparability. As one might expect, the range of time points, question wordings, and response formats is exceedingly wide. Even descriptive generalizations are hazardous in these circumstances, yet Coughlin proceeds to make them. In his analysis of support for specific kinds of programs, Coughlin only has even roughly comparable data for two program types (old-age pensions and nationalized health care). Yet, for the three remaining program types, when survey data were by his own admission unavailable or extremely fragmentary for over 60 percent of the comparison points, Coughlin proceeds anyway to use the data to support descriptive generalizations.

Beyond these frequency distributions, no more sophisticated analysis is offered—even at points where possible additional analyses virtually thrust themselves on the reader. For example, Coughlin "analyzes" several bivariate rank-order associations by visual inspection. My own visual inspection of some of these situations suggested much more moderate relationships than judged by Coughlin. If one thought the data good enough to use in the first place, rank-order correlations would, of course, not be subject to such subjective disagreement regarding their implications. Ultimately, then, for failings of both theory and method, this book offers little to recommend it.

Kim Quaile Hill

University of Houston, Clear Lake City

Poverty and Problem-Solving under Military Rule: The Urban Poor in Lima, Peru. By Henry A. Dietz. (Austin: University of Texas Press, 1980. Pp. xiv + 286. $19.95.)

This is a thoughtfully done study of the urban poor in Lima under Peru's "revolutionary" military government. Drawing on survey data, participant observation, and systematic comparison of six neighborhoods, Dietz contributes to both the Latin American literature on urban workers and the more general literature on political participation.

The survey data, approximately 550 interviews conducted in 1971, generally confirm earlier findings regarding the urban poor. A high proportion are migrants, but most hold non-marginal jobs. Most have achieved some upward social mobility, though Dietz emphasizes the shift from lower-level to more skilled manual occupations *after* arrival in the city. Most are relatively satisfied with their material progress and cautiously optimistic about the future. Very few blamed the government or existing social structures for their problems; most felt that "hard work" was the best way to improve their lot. These data obviously contradict the once-popular characterization of urban workers as an anomic, dislocated, and undifferentiated mass.

The second major theme is political participation by the urban poor. Whereas previous works have treated organizational membership as an independent variable, Dietz suggests that involvement in community self-help activities, membership in community organizations, and involvement in local and/or national demand-making are all distinct forms of participation. The first generally involves collective action at the neighborhood level to provide public goods, e.g., a neighborhood recreation center. The second, membership in the local organization, increases the probability of involvement with local or national authorities, but also includes other activities which are political but not demand-making per se. The argument for treating organizational membership as a separate mode of participation relies on factor analyses and causal modeling, about which I have reservations, but Dietz adds another element to the debate over the adequacy of the Verba and Nie typology of participation.

To me the more important contribution is the finding again that the dominant forms of participation are conventional, non-disruptive methods of petitioning and lobbying for assistance in the solution of local problems. The demands of the poor were generally limited to goods which the system is designed to produce. Often those demands were consciously tailored to the requirements of particular agencies.

The study also confirms the existence of a variety of participatory activities despite military rule, although this is an unusual case. The critique of SINAMOS and other mechanisms for participation created by the Velasco government adds to a growing literature on the political limitations of military radicalism in Peru.

The major weakness of this book is a certain diffuseness, which results from its attempt to be truly comprehensive in its approach to the urban poor, locating individual acts of participation within a specific community context as well as the larger national and international context in order to explore how the poor affect and are affected by the nature and structure of power within society. This is a herculean task, which necessarily involves a large number of related but nevertheless separate questions. Trying to deal with all of these —from the causes of urban migration to the limits of military radicalism—across all the levels of analysis involved—from individual differences among migrants to the structure of world capitalism—involves the author in many issues which go far beyond his data. A less ambitious study more closely tied to the original purposes of the field research would have produced much the same conclusions without diminishing Dietz's contributions to our understanding of the politics of the urban poor.

JOHN SAMUEL FITCH

University of Colorado

Political Development and Social Change in Libya. By Omar I. El Fathaly and Monte Palmer. (Lexington, Mass.: D.C. Heath, Lexington Books, 1980. Pp. xiii + 230. $13.00.)

Since they seized power in 1969, the overriding domestic preoccupation of Colonel Muammar Qadafi and Libya's other revolutionary leaders has been the search for a satisfactory political and economic system. The regime has had considerable revenue from oil at its disposal. Nevertheless, it has gone from one political experiment to another with only limited success. El Fathaly and Palmer describe and assess these efforts to achieve development objectives, most notably the mobilization and resocialization of the country's citizens and the transformation of Libya's rentier state economy. The book is thus about the nature and consequences of strategies of nation-building, fitting nicely into the emerging policy analysis perspective in the study of political development.

Libya's development efforts went through three phases between 1969 and 1979. The first was marked by the replacement of the traditional elite with modernizing administrators, by the creation of a mass-oriented political organization, patterned and named after Nasser's Arab Socialist Union, and by the use of schools and mass media as agents of socialization. However, Qadafi acknowledged the failure of these efforts in 1973 and offered new proposals to guide the revolution. Similarly, he stated in 1975 that progress was still "too random" and articulated in the *Green Book* the philosophy of an even more radical transformation. Part I, published in 1976, deals with political organization and Part II, appearing in 1978, concerns economic change.

El Fathaly and Palmer summarize the evolution of Libya's political strategy during the revolution's first decade and give critical assessments of specific programs. They show, for example, that modernizing administrators, appointed to bring the revolution to the masses, were in fact quite timid on matters of change. Other efforts assessed include the national plans of 1973 and 1975, which were hampered by a dearth of competent middle managers and which had the unintended consequence of reinforcing the rentier state pattern. The evaluation of early programs relies heavily on survey data and overlaps to some extent with *Political Development and Bureaucracy in Libya* (Heath, 1976), which the authors published with Richard Chackerian (reviewed in the *APSR,* September 1979). Other information sources include aggregate statistics, government documents, newspaper reports and interviews with local and national elites.

Beyond its theoretical interest in strategies of development, the book provides much useful information about contemporary Libya. The characteristics of the political elite are presented, for example. Also notable are translations from the *Green Book* and a detailed summary of the five-day General People's Congress of 1978.

A weakness of the volume is that the data with which it evaluates regime policies are sometimes thin. The accuracy of the authors' general conclusions is not at issue; what is needed is more specific factual information about the functioning and impact of government programs. On the other hand, given the difficulty of collecting reliable information on such sensitive matters, it is more reasonable to commend El Fathaly and Palmer for the data they do provide than to complain about what is missing. A related concern is the authors' apparent acceptance of the regime's view that a lack of popular motivation and revolutionary consciousness are among the major obstacles to progress in Libya. Thus while some Libyans may find the book overly critical, other observers may consider it too scrupulous in judging the revolution on its own terms, thereby ignoring the possibility that Libya would be better served by a different conception of development. But this is a subject for another investigation. El

Fathaly and Palmer assess policy by referring to the revolution's own goals and, in the process, offer a useful and thoughtful account of the nature and consequences of Libya's development strategy.

MARK TESSLER

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Ethnic Soldiers: State Security in Divided Societies. By Cynthia H. Enloe. (Athens: University of Georgia Press, 1980. Pp. xii + 276. $19.00).

The thud on the desk of a new book on either ethnicity or the military might evoke only a yawn. It would find many recent companions on the bookshelf. However, the reader's curiosity is at once aroused when the two themes are discovered interwoven. *Ethnic Soldiers* will well repay all those attracted by its novel topic.

By joining ethnic questions with military ones, Cynthia Enloe makes a breakthrough on both fronts. She provides important new insights concerning the politics of ethnicity by examining its operation in the security core of the state. At the same time, conventional views about the military are confounded by her careful inquest into the ethnic composition of military forces, concealed from view by conventional assumptions about the quintessentially national and integrative character of the armed forces. On both scores, the book will stand as a major contribution.

Enloe fired one of the first salvos in what became a barrage of ethnicity studies with her 1973 book, *Ethnic Conflict and Political Development.* The present volume moves beyond the earlier work in its theoretical sophistication and in the scope of materials used, both in time and space. Even the most skeptical reader cannot fail to be impressed by the range of her examples.

The most evocative and stimulating concept introduced by Enloe is the notion of a "state security map." Most contemporary (and for that matter historical) states are multi-cultural. While the unitary doctrine of nationhood is usually coupled with state creeds, in reality most ruling elites must quietly take cognizance of varying patterns of polity relationship among various ethnic communities. Cultural groups vary greatly in their degree of politization, in their affective attachments to the national entity, in the socioeconomic advantage (or deprivation) they derive from it, and in their proximity to the core institutions. Further, each of these factors is situational, fluid, and changeable over time.

Military establishments became an integral part of the institutional means by which the state copes with external adversaries, and also assures the hegemony of the state over the population. Because force is placed in the hands of the custodians of state security, the fidelity and reliability of those custodians has troubled rulers from time immemorial. Enloe brilliantly demonstrates how crucial ethnic calculus has been in controlling strategies for the security forces. What social composition must a constabulary have to repress uprisings among those groups where unrest is most likely?

From such ruminations comes what Enloe terms the "Gurkha syndrome," which was to have such a widespread effect on the composition of colonial armies. Particular communities, usually socially marginal, acquired the reputation of forming a "martial race." As colonial security forces became dominated by such groups, a military tradition could well arise in these recruiting areas, where—in the absence of appealing alternatives—army service became a way of life. More particularly, Enloe documents the impact of ethnic strategies in military personnel policies in countries as diverse as the United States, Canada, Peru, Russia, Britain, Malaysia (and many more).

Most studies of the military really focus on armies. Other components to the security establishment—police, navy, air force—are overlooked: but not by Enloe. Indeed, ethnic calculus operates quite differently in each of these domains, in function of the role played and status enjoyed. Police are generally of lower status, less well armed, and not a potential threat to the state itself; because of their more direct interactions with the populace, there are more advantages and fewer risks in representativeness. Navies and air forces, by contrast, are much higher-status pursuits, and for the most part lack the capacity to directly intervene internally; much more than armies, they are truly external security branches. Thus social class has been much more important than ethnic calculus in shaping the composition of these forces, especially their officer cadres.

Any undertaking of such scope inevitably invites the reviewer to carp at some omission. The volume loses some of its punch in the final portion, as the examples begin to become repetitious. Enloe asserts the importance of renascent theoretical concern with the state, but then fails to make use of it. She too abruptly dismisses the significance of regional shifts in regime dominance brought about by military coups in Benin and Congo-Brazzaville (pp. 143-58). And Lunda citizens (men as well as women) will take justifiable offense at being categorized as "tribesmen" (p. 231).

However, these are inconsequential quibbles. Enloe may take pride in the "ethnic soldiers" she marches past the reviewing stand. They are a well turned out unit, and merit our salute.

M. CRAWFORD YOUNG

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Poverty and Inequality in Common Market Countries. Edited by Vic George and Roger Lawson. (Boston: Routledge and Kegan Paul, 1980. Pp. xi + 253. $27.50, cloth; $15.00, paper.)

This collection of individual country studies seeks to answer three important questions: How is poverty defined in the various EEC countries, and what policies have been adopted to deal with the problem? How extensive is inequality in income and wealth among different social and economic groups? And what trends can be discerned in the understanding, treatment and prevalence of poverty and inequality? The countries dealt with are the United Kingdom, Belgium, France, Italy, Ireland and West Germany.

This book demonstrates most clearly the numerous difficulties of exploring, let alone answering, questions about poverty and inequality. Problems of data hamper not only comparative analysis, but also investigation of these issues in a single country. Nevertheless, all contributors make a valiant effort to use what reliable data are available, and to arrive at conclusions concerning both the extent of poverty and inequality and the effect of governmental policies on them. The data for all of the countries surveyed point to similar conclusions. Despite unprecedented high rates of growth both in the economy and in government spending over the past few decades, considerable income inequality persists. Social welfare and tax policies have done little to reduce the extent of socioeconomic stratification. If anything, they have tended to reinforce income inequality. Few inroads, moreover, have been made in lessening the amount of poverty in many of the Common Market countries. In most instances, income maintenance programs have not been aimed at low-income groups; consequently, they have not benefited from such programs. Finally, almost all contributors suggest that the problems of inequality and poverty are interrelated, and that if there is no effort to deal effectively with the former, attempts to reduce the latter will meet with only partial success.

In the studies of individual countries, however, the extent to which the question of inequality has entered into the political arena is paid scant attention. Indeed, with the exception of the discussions of France by Adrian Sinfield and on Italy by David Moss and Ernesta Rogers, there is little systematic analysis of either the ideological or the political context in which poverty and inequality are imbedded and maintained. A discussion of various functional and structural theories of poverty and inequality is found in the introduction to the volume by Vic George. But it is too general to provide testable hypotheses concerning the extent and nature of inequality and poverty in the EEC countries or differences and similarities among them.

Equally lacking is a systematic comparative analysis of these differences and similarities. Roger Lawson and Vic George, in the concluding chapter, offer an overview of the findings from the single-country studies. They attempt to rank the countries according to level of income inequality and, more problematically, according to degree of poverty. They also suggest that, both across and within countries, "The relative level and changes in income maintenance expenditures do not bear much relationship to the extent or change in relative poverty" (p. 240). They attribute this to the existence of other more powerful structural and economic determeinants of inequality and poverty, as well as to the fact that the beneficiaries of such expenditures have not been the most disadvantaged. Yet a comparison of these determinants and of the impact of social welfare policies is not made. Also not addressed in this volume are the subjective dimensions of inequality and poverty; that is, people's awareness and evaluations of existing patterns of stratification. This, Sinfield argues, "might help to account for different patterns of poverty and the extent or lack of policy concern for the deprivation of particular groups" (p. 117).

Despite these omissions, this work does contribute to our knowledge of the extent and nature of inequality and poverty in certain European countries, and does lay the groundwork for further more systematic comparative analyses of these phenomena and the reasons for their persistence.

GOLDIE SHABAD

Ohio State University

Race and State in Capitalist Development: Comparative Perspectives. By Stanley B. Greenberg. (New Haven: Yale University Press, 1980. Pp. xiii + 489. $40.00, cloth; $11.50, paper.)

This is a densely rich and provocative book. Stanley Greenberg traces the emergence of capitalist agriculture and industry in the U.S. South (specifically Alabama) and in South Africa to answer the important political question: does the rise of capitalism necessarily produce a free labor market, one in which racial and ethnic categories and barriers fall away? Using the cases of Alabama and South Africa—and, to a lesser extent, Northern Ireland and Israel—Greenberg challenges the facile assumption that capitalists and their state allies need and, where they are powerful, create a free labor market and, in so doing, make racism more and more anachronistic. Such an assumption misses the very overlaps and lags that create political contradictions and the structures intended to resolve or at least band-aid over those contradictions.

Much of the analysis of the transition from feudalism to capitalism within societies and as prompted by international pressure is peculiarly apolitical. The macro-analysis that portrays certain transformations as virtually irresistible essentially denies the existence of choices, of selective use of power. That is, in many of even the best studies of the history of political economies, the *politics* of the evolutionary pattern is left out. It is as if no one ever made choices, as if there were no choices to be made. This stands out as especially paradoxical in that some of the best work, by analysts such as Emmanual Wallerstein, Perry Anderson, and Samir Amin, is intended to be apolitical critique of the current international power configuration. Greenberg makes strides toward reintroducing politics by pin-pointing those choices, actors and legal contrivances that characterized the usually contradiction-filled transition period. It is important for political scientists to take a hard look at Greenberg's book, to incorporate into their own research both the macro-historical sensitivity (too often lacking in much of the work in our discipline) and the specifics of power utilization.

Greenberg makes clear that in both South Africa and in the U.S. South, as well as in Ulster and Israel, it is impossible to comprehend the peculiar character of the contradictions in the feudalism-to-capitalism tradition without taking explicit account of two politically charged phenomena: ethnicity and the state. Actually, Greenberg discusses racism more than ethnicity, and when he compares South Africa's and Alabama's capitalization of agriculture and industrialization with those of Ulster and Israel it would have been helpful to have drawn finer distinctions between racism and ethnic communalism. Nonetheless, he does demonstrate that, in reality, the alleged capitalist imperative of "free," mobile labor is tempered by the utility of racial/ethnic categories for even the emergent capitalists. Furthermore, his comparative study exposes the state's crucial part in not just preserving such categorizations, but also in adjusting them so that the South African mining companies will get the African laborers they need and the Birmingham steel mills will get the racially structured work forces they need—at the wage rates and under contract conditions that will assure not only profits but also labor stability. The state, according to Greenberg, is not a passive tool in the hands of the capitalist class, but rather is often an active resolver of tensions between capitalists in different sectors with different labor/ethnic needs and between the emergent capitalists and the older feudal agrarian elites. Rarely, of course, do the various sectors of the labor force—white or black, Protestant or Catholic, Arab or Jew—control that state apparatus.

This book makes a contribution to political science by pointing to the ways in which the state evolves during the early capitalist period in ethnically and racially segmented societies. It compels us to take legal structures seriously, to analyze them as means (not always successful means) by which certain classes attempt to alter the mode of production to suit their own interests without giving up those racially and ethnically defined relationships that have bolstered their influence in the past.

At the end, the reader is left a bit uncertain as to whether Greenberg believes that "ultimately" the transition will pass; if (a big, i.e., politically problematic, "if") the state is astute enough to first patch over and then resolve the contradictions during the lag period, then indeed the imperatives of capitalism for a "free" labor market will rule, and racial and ethnic barriers will wither away. Because of his interest in both U.S. southern politics and South African politics, Greenberg has a real stake in projecting their respective trends. He certainly is not a sanguine political observer of either of these systems. But his conclusion could be read as coming down, even if tentatively, on the side of projections which assume that more genuine capitalism will finally produce an awareness on the parts of both the strongest business interests, and the state elites who have become their allies, that racial discrimination, even racial stratification (the two are not synonymous), are anomalous. One looks forward to Greenberg's continuing monitoring of both these political economies, for he has the comparative skills and historical sensitivity to make us smarter about the ongoing processes of change and contradiction in capitalist states.

CYNTHIA H. ENLOE

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Politics and Policy Implementation in the Third World. Edited by Merilee S. Grindle. (Princeton, N.J.: Princeton University Press, 1980. Pp. xv + 310. $20.00, cloth; $7.95, paper.)

Implementation, the relative success or failure of public policies, has emerged as a focus for administrative studies in the past decade. Implementation analysis might supercede in popularity the grander schemes of managerial science for the selection of policy alternatives such as cost-benefit and systems analysis. The emergence of implementation stems from the failures of public policy on a global scale—from New York City to terror-stricken Uganda. Aspirations and ambitions are in retreat. Now the most important criterion for a policy is its feasibility. Can the policy alternative adopted be implemented, achieving the goals in view?

This conceptual approach is applied in this series of nine case studies in Zambia, Peru, India, Colombia, Brazil, and Kenya. The context (the political environment of the policy) and the content (types of benefits—e.g., housing, nutrition—interests involved, resource commitments) are examined for their consequences for implementation. Merilee Grindle states that interest in implementation has been stimulated by the realization that the translation of policy statements into goals, whether the implementation is successful or not, involves "fundamental questions about conflict, decision-making, and 'who gets what in a society' " (p. 3). Implementation, then, is ultimately a political process.

This book undertakes a difficult task. The conceptual framework is an effort to reduce an irrational process having greatly disparate goals, e.g., squatter removal, infant nutrition, rural electrification, agricultural cooperatives pursued in different historical and cultural contexts, to a model which relates complex and often ephemeral real-world events to specific variables. Accomplishing this, given the present state of the art, is for political scientists the equivalent of the mathematicians' squaring of the circle.

This volume deserves consideration from two perspectives: (1) To what extent does the conceptual apparatus of implementation contribute to the outcomes in the case studies? Does implementation provide a basis for classifying the events and relationships which account for success or failure of the programs as designed? (2) Do the case studies contribute to the understanding of administration of social and economic change in the Third World?

In the concluding chapter, "Implementation amidst Scarcity and Apathy: Political Power and Policy Design," Peter S. Cleaves appropriately answers the first question. He places these materials, after observing that the success or failure of program "depends upon the perspective of the observer" (p. 281), into a framework of closed, intermediate, and open systems and "Coordinates Affecting Policy Implementation." These coordinates are policy actors that have more or fewer resources dealing with policies that are more or less problematical. Cleaves tells us that whether or not a policy can be implemented depends upon whether the regime is relatively authoritarian and coercive, or democratic and persuasive, and whether the implementers of policy are rich or poor in resources to apply to policies that are tough or easy to implement.

Considering the book as an instance of implementation, the application of a theoretical design to complex data, I find that these conclusions, after elephantine labors, bring forth a mouse. This is not to denigrate the intellectual effort or the validity of the conclusions. However, an experienced student of government administration could have arrived at these conclusions about the obstacles to implementation without the aid of the model.

The second question involves the informative value of the case studies. All of them, but most notably David F. Pyle's study of a pilot infant-nutrition project, left the impression that the authors were writing with inordinate restraint. They appear to be telling less than they know. There are several plausible reasons for this: conventional scholarly caution, the fact that five of the studies had their origin in dissertations with one in progress, or constraints introduced by the conceptual framework.

The case studies are adequate and informative. Of signal interest are Stephen A. Quick's report on ideological molding of implementation and its consequences in Zambia, and Frederick T. Temple's and Nelle W. Temple's report on how a public housing program in Kenya became the source of spoils for a favored few. Merilee S. Grindle, in her analysis of the role of "the implementor" in Mexico, gives the most comprehensive and disciplined application of the theoretical framework. She also provides an extensive bibliography of the literature of implementation.

In her introductory chapter, Grindle observes: "Most Third World countries have extensive and active public sectors involved in many aspects of economic and social life, and state bureaucracies may be among the strongest institutions of society" (p. 17). Later in the chapter she says: "The functions of public administration, and thus the capacity to carry out program activities, are characteristically not well institutionalized in Third World countries" (pp. 25-26). Pondering these two propositions can support the thought that implementation is a revival of the much-abused politics/administration dichotomy, with the impiety of profane politics as always destroying the prospects of salvation.

HERBERT G. WILCOX

West Virginia University

Public Policy in a No-Party State: Spanish Planning and Budgeting in the Twilight of the Franquist Era. By Richard Gunther. (Berkeley: University of California Press, 1980. Pp. xiv + 361. $17.50.)

The study of corporate authoritarianism has yielded a variety of fresh ideas and insights that bear on our theories of the state, political economy and interest-group representation. Thus, while the regime of Francisco Franco has passed into history, important questions about its implications and significance linger on.

Richard Gunther's book is a valuable effort to give a more precise and informed answer to a question that guided several earlier studies of public policy making in late Franco Spain. How different in fact are the processes of economic policy making in authoritarian and democratic systems? In the last generation of Franco's rule, Spain adopted a program of economic management that closely paralleled the strategies of other Western capitalist nations. Examining the record of this period, can we determine how important democratic institutions are in steering an industrial economy? Was Spain hampered—or advantaged—in its pursuit of the goals of modern economic policy because of its peculiar political arrangements?

Gunther's case is that authoritarianism did make a difference. Budgetary policy in late Franco Spain was made in something of a power vacuum. The Caudillo provided little systematic guidance in economic and social policy. His concerns were mainly with regime structure and maintenance. The putative dominant party had become a hollow shell, served in no sense as a vehicle for interest aggregation, provided no agenda for public policy. Neither legislative process nor interest-group activity had much bearing on the policy process. Consequently, the budgetary process was highly legalistic, hierarchical and personalistic. The politics of the budget were the politics of the administrative state.

Within a "zone of indifference," Franco's economic advisers had substantial discretion to set the economic goals and policies of the regime. Nevertheless, they operated in a space delimited by certain identifiable political constraints set by the Franquist elites. Thou shalt not increase taxes. Thou shalt not issue public debt. Thou shalt not generate revenues by means of the printing press. This meant that the economic managers were precluded from using certain Kenyesian measures fully to steer the economy, and relied more heavily on a policy of deregulation, indicative planning and selective public investment.

In the end, Gunther argues, the structural characteristics of the regime did delimit the agenda for change. Proponents of more substantial reform were prohibited from organizing public support behind their proposals. Unless the Caudillo perceived a relation between economic programs and his own goals of regime stability, policy innovation could not be effectively pursued within the channels of the administrative state. The result, for Gunther, was an agenda and a policy product quite different from what might have been possible in a more democratic context.

In making this case, Gunther seems to be arguing against certain propositions—perhaps, more generally, the tone—of my earlier study of economic policy making in Franco Spain. It is true that I was impressed by the range of proposals and initiatives that were admissible into Spanish political economic discourse in the 1960s. I still believe that it is necessary to understand the dynamics of the economic policy debate in this period to appreciate certain continuities in the policy-making processes of late Franco and post-Franco Spain.

Nonetheless, I think Gunther offers a needed critique and corrective to my earlier views. He is right in emphasizing the constraints and rigidities against my interest in the latitude that was available for what Albert Hirschman called "reformongering." At times, however, I think Gunther unnecssarily sharpens the differences in our perspectives. (For example, at p. 92, he takes exception to a "characterization" of Spanish policy making in my book which is actually a statement about Western policy making generally.) In fact, our interpretations differ mainly in nuance and detail. We are not arguing totally different theses.

This is a carefully researched and well-written book. It is in several ways the most successful study of economic policy making in Franco Spain. Medhurst's work was too descriptive and preoccupied with administrative detail, my own too narrow in focus and too much guided by a somewhat idiosyncratic theoretical framework. Gunther has the advantage of perspective and he captures the overview. This is a straightforward and comprehensive portrait of the way in which the distinctive politics, institutional framework and norms of a corporate authoritarian state affected its decision-making processes.

CHARLES W. ANDERSON

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Pluralism and Corporatism: The Political Evolution of Modern Democracies. By Reginald J. Harrison. (Winchester, Mass.: Allen and Unwin, 1980. Pp. 198. $22.50, cloth; $9.95, paper.)

Reginald Harrison is concerned with the present and future state of modern democracies. He argues that democratic politics in modern industrial societies more closely resembles a corporatist model than a pluralistic one.

In this corporatist model authority in different decision areas was delegated to non-competitive, governmentally linked, consultative bodies "in which managerial, professional and union technocrats met with the appropriate government bureaucracies as the recognized representatives of groups within their sector and had an important, often decisive, influence on policies affecting their sector" (p. 29). Corporatism is a "collaborative but functionally segmented process of policy formation" (p. 188) in which monopolistic corporate interests control access as well as policy.

Harrison seeks to explain the reasons for the growing importance of corporatist modes of decision making and to indicate the implications for contemporary democracies. The book takes the form of an extended essay, with illustrative references to structures and events in a number of Western European countries and the United States.

This analysis finds the origins of corporatist predominance in the ideological consensus and economic prosperity of the 1950s and 1960s. With widespread agreement on social goals and confidence in general economic policy directions, the policy-making process seemed largely one of technical application, appropriately left to governmental bureaucracies. The bureaucracies in turn were encouraged to secure the agreement and cooperation of elites in various social sectors by permitting their participation—and even predominance over—decision making in their sector.

Harrison views the consequences of corporatism in contemporary democracies as doubly negative. One consequence is a decline in participation, representation and accountability in politics. A second implication is poor economic performance. Both the radical protests of the late 1960s and the sluggish industrial performances of the 1970s are presented as primarily the results of corporatist predominance in decision making.

Drawing on the work of Hayek and Galbraith, Harrison depicts the contemporary "mixed" economies of the industrial West as inherently flawed. "The absence of either purposeful central government coordination or of any automatic regulator like competitive market pricing help account for economic problems. . . . The absence of a coordinating mechanism means that the corporations, seeking to regulate their environment, are quite likely to be regulating their own failure" (p. 187). Unfortunately, the analysis does not attempt to assess the simultaneous impact of other domestic and international factors on economic performance. Nor does it explain the relative economic success of some of the countries with highly developed corporatist tendencies (such as Austria and Norway), in comparison to less corporatist systems such as the U.S.A. Rather, British economic problems are placed at the doorstep of corporatism, and ad hoc examples from other countries are used to assign general corporatist responsibility for poor economic performance. The problems of "pure" market and "pure" centralized systems are not discussed.

Harrison is aware of the diverse historical factors shaping the adoption of corporatist decision making in different societies. However, despite the book's subtitle, the historical processes are not emphasized here. The reader is well recommended to turn to the relevant essays of Lehmbruch and Schmitter. Harrison does examine the role of parties and constitutions in limiting corporatist tendencies. In his view parties provide some potential counterweight to corporatist closure in decision making. He points out the dilemma involved in asking party systems to provide both completely open access to power for new groups and an accountable decision process through which established elites can be collectively controlled. Harrison's reflections on parties and constitutions lead him to favor the French and American systems, with loose parties and strong presidencies (p. 112). This preference is not, however, grounded in serious empirical investigation of multiple causes of corporatism or even its pervasiveness in the respective societies. For control of the bureaucracy, the Swedish model is held up for emulation.

In short, *Pluralism and Corporatism* provides a reasonable introduction to the corporatist analytic approach, whose influence has been growing in recent years, and tries to place it in the context of more traditional thought about parties and constitutions. It can thus render a useful service for those who have not yet been exposed to the stimulating insights of the corporatist literature. However, as presented here, its generalizations often seem simplistic in tracing both causes and, especially, consequences of corporatist decision making. Suggestions about linkages are usually plausible, but are not buttressed by rigorous consideration of multiple factors or by systematic empirical comparison. Nor is serious account taken of alternative or complementary explanations of economic and political discontent in contemporary democracies.

G. BINGHAM POWELL, JR.

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Soviet Politics, Political Science and Reform. By Ronald J. Hill. (White Plains, N.Y.: M.E. Sharpe, 1980. Pp. x + 221. $27.50.)

Over the last 15 years, Soviet scholars and commentators have somewhat hesitantly begun to use the term "political science" to describe a valid field of inquiry. Indeed, Soviet participation in the International Political Science Association began in the mid-1950s, a relationship sustained through the Soviet Political Science Association. And yet, there are still no departments of political science in the Soviet Union, or research institutions that bear this label.

Does political science as the West knows it exist in any form in the Soviet Union, however limited? Or is political inquiry limited to fairly narrow legal and institutional research, the exegesis of unchallengeable Marxist-Leninist principles, and bits and pieces of sociological inquiry? Ronald J. Hill has sifted through a mass of Soviet scholarship and documents in pursuit of at least a tentative answer.

One cannot, of course, deny that Soviet research in political institutions is valid political science, since it was not so many decades ago that Western political science was using analogous institutional research as a primary focus. Hill directed his search toward the Soviet use of elementary theoretical concepts, including information, opinion, function, and role. His text is not entirely structured around these concepts, however, which would have made a difficult task at best. His chapter headings divide the inquiry between institutions and concepts: the electoral system, the soviet deputy, and local government reform, a chapter on "Interest, Information and Public Opinion," followed by "Intra-Party Democratization." The findings are summarized in two concluding chapters, one on the process of political reform, and one on the larger problem of political change.

What did the author find? Under each topic, something of interest did indeed turn up. In no area did Hill's search turn up wholly negative results. Regarding the electoral process, for example, several Soviet scholars have suggested procedures for insuring genuine secrecy of the ballot, in place of the long-standing practice of offering curtained polling booths only for the purpose of striking names from the one-candidate slate. On the other hand, there have been only a couple of hints of interest in multiple candidacies, and this in spite of the fact that multiple candidacy has long existed in parts of Eastern Europe. For the most part, Soviet scholars' proposals for reforming local government deal with such topics as how to assure meaningful reporting from local soviet deputies to their constituents, surveys of how deputies apportion their time, how to involve the public in the sessions of local soviets, and what to do about the virtually moribund village soviets. Few of the proposed changes would do anything more than streamline the existing system. The most radical proposal, which actually dates back to the 1950s, would put local administrative departments under the direct supervision of commissions of deputies, thereby erasing the formal distinction between legislative and executive functions, and realizing a feature of the 1871 Paris Commune pointed out by Karl Marx.

What of the Communist party? The one really new development since the mid-1960s is the fact that investigation of the relationship between party and state is now both accepted and promoted. The proposals that have emerged from research in this area, however, focus on delimiting the party's functions by setting forth in detail the roles that party officials and members should and should not play. Discussion of this problem is by itself nothing new; the point is merely that published research has advanced beyond the old generalities. Policy making, leadership and overall supervision remain the realm of the party, which leaves legislation and policy application to the organs of government. The chapter heading "Intra-Party Democratization," is a misnomer, since the text yields almost nothing about democratization in the sense of increasing the scope of electoral and representative procedures. Instead, as Hill himself concludes, the entire emphasis is on greater efficiency and rationalization for the existing system.

Only in the chapter "Interest, Information and Public Opinion" has the author discovered lines of Soviet inquiry which, if they ever take root in the Soviet Union, point to significant change. The concept of "interest" is now well established, certain types of interest conflicts are accepted and studied, and some forms of interest articulation are a subject for research. At least one bold writer (Shakhnazarov) has made a case for greatly liberalized circulation of information.

The book ends on a note of cautious hope for expansion of these trends in political research. From their present involvement in local reform and experiments, Soviet scholars may well move into comparative research among communist nations. Beyond this, the change of political generations may open up further doors. However, Soviet scholarship in this area (as Hill himself concedes) has little impact on either the public or the top leadership. Hill's hopeful conclusions about the future of the young political science profession are somewhat dimmed by the realization that if political change occurs in the coming decades, the present generation of scholars seems fated to follow rather than help to generate it.

ROBERT J. OSBORN

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Soviet Leadership in Transition. By Jerry F. Hough. (Washington, D.C.: Brookings Institution, 1980. Pp. xi + 175. $11.95, cloth; $4.95, paper.)

In his *Soviet Leadership in Transition,* Jerry Hough has presented a fascinating picture of the accomplishments and attitudes of the leadership of the Soviet Union. During World War I, Russia was one of the more backward countries; its population only mediocre in health, its economic efficiency poor and its standard of living low. In World War I, a much smaller Germany and Austria were able to occupy the richest parts of Russia, and had it not been for the U.S. entrance into the war, might have won and occupied parts of Russia permanently or converted them into client states. Yet in that same period, Russia could boast novels, poetry and music that had won world acclaim. Now, by comparison with the most advanced countries of the world, the Soviet Union's public health is good; it is a great, although hardly the most efficient economic power; the standard of living is much improved; and it is one of the two greatest military powers in the world. How this great change took place is the subject of Hough's thoughtful study.

As with most of humanity, our judgments are rooted in our own experience. We tend to see the industrialization and modernization of Western Europe and some of its colonies as the only possible model, which is in fact far from true. The rapid and extensive progress of Japan yesterday and China today and tomorrow illustrates how special the Western European experience is. The Russian experience has its own unique qualities.

Russia was, and to some extent, still is different from Western Europe and its advanced colonial outposts. The Russian standard of living, and the literacy and technical skills of the bulk of the population were considerably lower than those of advanced countries. But the differences were in degree rather than kind. Even with many of the best-educated and most highly skilled people either leaving the country during the civil war or expelled or imprisoned by the Soviet leadership, in a relatively short time the Soviet Union advanced enormously, with dramatic improvements in living standards and skills of the population. Although the amenities of refined existence are still in short supply, the Soviet Union is not a backward country. The educational level has risen enormously. The scientific establishment, unlike its Tsarist predecessor, concentrates on practical problems rather than pure science. The industrial and political machinery of the Soviet Union, for all its lack of polish and refinement, works well. Its population, whatever their grievances, made loyal and effective troops against a superior foe in World War II. Foreigners from backward countries find much to admire in the Soviet Union, though they may find some of the luxuries of London and New York lacking.

Hough deals with the important aspects of the question of how Russia, which approached the task of modernization belatedly, managed to catch up so quickly and extensively. The work examines mass standards, the administrative elite, the military and foreign policy establishment, economic activities, and the changes in experience and attitudes from one generation to the next. The Soviet Union has gone through and is still undergoing the crisis of industrial and educational modernization in a political regime that has scant regard for the rights of individual expression and thought. Much remains to be done, and from the perspective of the wealthy in advanced Western nations, life in the Soviet Union is not very appealing. Yet in its self-conscious modernization, the Soviet Union has accomplished much, and Hough's book adds greatly to our understanding of that development.

HERBERT S. DINERSTEIN

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Beyond Ujamaa in Tanzania: Underdevelopment and an Uncaptured Peasantry. By Goran Hyden. (Berkeley: University of California Press, 1980. Pp. x + 270. $18.50, cloth; $7.95, paper.)

The Tanzanian experience has held a special fascination for the past 15 years. For many it has been an experiment in creating socialism without totalitarianism; for others it has been important as a development model focusing on rural agriculture, rather than unavailable, capital-intensive, industrial technologies. Politically, Tanzania is a nation where corruption and individual greed have not destroyed the lofty ideals of the independence struggle. Seeking self-reliance and autonomy from the fortunes of the international capitalist system and great power struggles, Nyerere's Tanzania sought to organize development in terms of locally based village development (*ujamaa*), relying on labor-intensive practices and political organization to create rural production units to transform the society. Foreign capital, either in the form of capitalistic investment or foreign aid, was not to be the core of Tanzania's plan and was to be accepted only under certain limited conditions.

Evaluating this course of action is not as easy as some would like it to be. Part of the difficulty is that we first need to figure out what happened in Tanzania since the Arusha Declaration. From this point of view, it is clear that Tanzania neither developed as its leaders wanted, nor collapsed as its critics claimed it would. In both cases there were only errors in predicting macro-outcomes, but more interestingly from the point of view of social science, these are not just due to outside forces such as natural disasters, war, or the rise in the cost of petroleum products. They are also due to lack of understanding about how social, political and economic activities are related inside Tanzania.

Hyden's book is a fascinating answer to this question. He says the Marxists are wrong because they overestimated the impact of dependency in Tanzania; it is neither international capitalism nor petty-bourgeois capitalism at the local level which has impeded Tanzania's effort. Nor is the problem bureaucratic incompetence and nepotism. Rather, Hyden argues that the peasant mode of production in Tanzania is essentially pre-capitalist and sufficiently strong to resist the building of capitalism or socialism, at least for the moment. Their mode of production gives rise to an "economy of affection" which provides the basis for social and political actions which are quite different from those sought by the national leaders. Peasant independence comes about because the government has too little to give the peasant as an incentive for doing what it wants and because what the peasants want is not what the government wants.

In making his argument, Hyden surveys conflict between the government and the peasantry in Tanzania since the onset of the colonial period. First under the Germans, then under the British and finally after independence, peasants have been seen as uncooperative and lazy. They don't do what they are told, and what is more, they don't do what is good for themselves. But Hyden argues that they are not so irrational. Rather, they have managed to maintain their autonomy and continue to promote what means most to them—the values embodied in the economy of affection. Not only does the pre-capitalist economy of affection make mobilization of peasants difficult, if not impossible, but furthermore this mode of organization incorporates many urban workers and members of the petty bourgeoisie who maintain extensive ties to their families in the countryside and continue to own land there as well. These people are "capable of organizing alternative means of sustaining their livelihood. . . . Thus not only are the peasants in Tanzania capable of ignoring state demands but so are the state's own employees" (p. 170).

The small is not so beautiful in Africa, Hyden writes, and for development to take place, contradiction and conflict between the peasantry and those who control the state are required. The rise of modern socialism in Africa means that present means of production which are both pre-capitalist and pre-socialist will have to be destroyed. "As long as the rural producers are absorbed by the social logic of the peasant mode, there is little room for modern socialism" (p. 221). Furthermore, accomplishing the task will require the same use of force that capitalist modernization did. Thus "development in a direction towards greater national self-reliance is improbable without a successful subordination of the peasantry to the demands of the ruling class, whether they are identified as 'petty bourgeois' or 'revolutionary.' . . . Such is the development logic in contemporary Africa. Nobody ought to be surprised if the peasants object to the logic of the process" (p. 232).

I find the argument carefully developed and well illustrated. While some of the strands are difficult to discern at first, they are well woven together by the end. The conclusion, which is certainly not the only one which can be drawn from Hyden's argument, is controversial but provocative as well. He reminds us that good intentions are not enough and that structural conditions cannot be ignored.

MARC HOWARD ROSS

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Labour and Scottish Nationalism. By Michael Keating and David Bleiman. (Atlantic Highlands, N.J.: Humanities Press, 1980. Pp. xii + 215. $37.50.)

Labour and Scottish Nationalism by Michael Keating and David Bleiman is an important addition to the literature on nationalism. Not only does it present a well-documented discussion of the historical connections between trade unions and nationalism in Scotland, but also a detailed case study of a socialist party and its reconciliation with nationalism. Thus the book makes a significant twofold contribution.

Keating and Bleiman are primarily concerned with developing these topiics, not in delineating the history of the Scottish nationalist movement.

They focus on how trade unions and the Labour party have dealt with the issue of home rule and its later derivative, devolution. In detailing those relationships in the early chapters, however, they present a substantial amount of information not readily available elsewhere, particularly for the period 1880-1930. For example, the Scottish Home Rule Society and the various phases of its existence are given extensive consideration. Also, attention is devoted to conflicts within the labour movement itself as different unions and individuals have competed for leadership positions. Their stances on nationalism have frequently been a factor in such bids for power. Much of the information on which these sections are based is drawn from contemporary union records and personal accounts, which contribute greatly to the substance of the work.

Keating and Bleiman found that the commitment to home rule within the unions and the Labour party has undergone circuitous shifts over the years. Initially, there was substantial support for home rule among trade unionists in Scotland and, consequently, among Labour party supporters. Then, as economic conditions worsened after the First World War, making industry-wide unions more desirable than smaller regional ones, and as the viability of a Labour government became more realistic, the depth of the commitment was reconsidered. The focus of support became one of securing more administrative discretion for Scotland than of a separate parliament or dominion status.

In the authors' opinion, it would be wrong to regard these changes as having been made for simplistic electoral or economic advantage. The Labour party and the trade union movement have never had the means for forging a completely consistent ideology. This has meant accepting policies favoring centralized economic planning, while also nominally supporting more localized decision making for Scotland. Any contradictions between these two goals was moderated greatly by the undefined nature of home rule/devolution, the assumption that Labour would govern both Scotland and the rest of the United Kingdom simultaneously, and unpublicized acceptance of the Scottish Office's right to act as Scotland's lobbyist at Whitehall. It is when these factors were altered by the electoral success of the Scottish Nationalist party in the 1960s and 1970s that the Labour party was forced to reevaluate its position on devolution. This matter of reevaluation of support for goals is an important aspect of the book, having theoretical implications beyond this topic. As Keating and Bleiman clearly show, the choice of which goal is to receive current emphasis depends upon many factors, only one of which is concern for electoral consequences. Personalities, economic conditions, historical commitments, and strategies of action, as well as events and conditions external to domestic political control, may have direct impacts on a party or a unions decisions. By illustrating this fact, the authors have notably expanded our understanding of how and why the Labour party has responded to the Scottish nationalists.

As I have stated, this book makes a significant contribution to our understanding of the development of Scottish nationalism, the history of Scottish trade unions, the Labour party, and the theoretical relationship between socialism and nationalism. It is not always easy reading, nor always even in its coverage. The highly detailed early chapters are often heavy going and stand in contrast to chapters dealing with contemporary events. Nevertheless, the book is both substantively and theoretically important. Readers interested in Scotland, British politics, or nationalism should be familiar with this work.

ROBERT J. THOMPSON

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Political Participation in Korea: Democracy, Mobilization and Stability. Edited by Chong Lim Kim. (Santa Barbara, Calif.: CLIO Books, 1980. Pp. xiv + 238. $26.50.)

This book, according to its editor, Chong Lim Kim, intends to contribute to the emerging field of political participation in Korea and to provide a systematic introduction to comparative political participation studies for the case of Korean development (p. xiii). In fulfilling both of these purposes, the book makes a significant contribution.

Broadly defined, political participation as a key political concept means "the participation of individuals and groups in the governing process, supportive and subversive, voluntary and involuntary, involving real choice and no choice" (p. 5). Political participation studies should include inquiries on the socioeconomic and personality dynamics of participants, on the various forms of participation in which they engage, and on the consequences of their participation. The book's wide range, therefore, includes topics as diverse as "Korean Political Participation as Collective Action" (Roger W. Benjamin); "The Modes of Citizen Political Participation" (Chong Lim Kim, Young Whan Kihl and Seong-tong Pai); "The Dynamics of Electoral Politics: Social Development, Political Participation and Manipulation of Electoral Laws" (Jae-on Kim and B. C. Koh); "Political Roles and Participation of Community Notables" (Young Whan Kihl); "Political Participation and Mobilized Voting" (Chong Lim

Kim); "Student Activism: A Comparison between the 1960 Uprising and the 1971 Protest Movement" (Sungjoo Han); "Environment, Policy, and Electoral Participation: A Comparison of Urban and Rural Areas" (Sung-il Choi and Chae-jin Lee); and "Constituency Service among Korean National Assemblymen: A Study of Leadership Responsiveness" (Chong Lim Kim and Seong-tong Pai). Some of these papers were initially presented at a conference held at the University of Washington in February 1975 that was supported by a grant from the Joint Committee on Korean Studies of the Social Science Research Council and the American Council of Learned Societies. Most of the other papers were contributed by the editor.

All of the papers show evidence of careful research as well as conceptual and methodological sophistication. The breadth and scope of the book, however, tends to make each paper stand somewhat by itself without a high degree of integration. Very often, furthermore, the studies work from variable data bases and time frames and cannot avoid some amount of repetition. Because empirical studies are limited by the restricted availability of data, the papers leave many questions unanswered about the nature and scope of political participation in Korea.

Some of the findings are, however, worth summarizing. They include: (1) Higher voter turnouts as shown in Korea are not necessarily a measure of democratic participation (Kim); (2) Rural voter turnout is always higher than urban voter turnout (Kim and Koh); (3) Urbanization correlates with lower voter turnout and a general turning away from support of the government party (Kim and Koh); (4) Regionalism modifies to some extent urban/rural differences in voting and party support (Kim and Koh); (5) Governmental manipulation of the less sophisticated rural voters is extensive (Kim); (6) The mobilized voting bloc is characteristically elderly, female, poorly educated, rural, and low in status (Kim); (7) Participation is hierarchical, depending on how easy it is (Kim, Kihl and Pai); (8) The regime relies on mobilized participation for legitimacy (Kim); (9) Electoral laws are often designed to neutralize anti-government forces (Kim and Koh); (10) Community notables or *yuji* serve as a link between citizens and politicians (Kihl); (11) Students are the only check on Korea's authoritarian regime, and student dissent has become better organized and more radical in goals (Han); (12) Public policies have more influence on urban than on rural electoral participation (Choi and Lee); (13) Korean voting behavior may still reflect the voter's rational choice (Benjamin); and (14) The greater the existing volume of demands on National Assembly members, the greater will be the service provided (Kim and Pai).

These findings may sound familiar to students of Korean and comparative Third-World politics. Perhaps a thorough investigation of the literature in the field could have helped delineate such findings in this volume. The papers have, however, placed these findings in a systematic and empirical framework through the judicious editorial work of Chong Lim Kim, and in this particular regard, the book's contribution is more than sustainable.

This book also challenges us to ask many questions regarding political participation in particular and political development in general. Some unanswered questions concern the use of money and the role of media in electoral politics. Other questions should include why some candidates for National Assembly are successful and some are not. The turnover rate of Assembly members has been relatively high in Korea. Future studies may also include not only mass electoral participation but also more extra-constitutional and functional group participation in decision making in an authoritarian, developing political system as represented by the Republic of Korea.

C. I. EUGENE KIM

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The Foreign Policy of West Germany: Formation and Contents. Edited by Ekkehart Krippendorf and Volker Rittberger. (Beverly Hills, Calif.: Sage Publications, 1980. Pp. 372. $20.00, cloth; $9.95, paper.)

This volume, number 4 in the series "German Political Studies" sponsored by the German Political Science Association, might prove of interest to the conservative foreign policy makers clustered around the new Reagan administration in Washington. Although their ideological views will not be in tune with the radical left views of many—but not all—of the young German political scientists who wrote these original essays, the American policy makers may have to contend with such views should the left wing of the Social Democratic party again put its mark on the foreign policy design of Chancellor Helmut Schmidt.

Actually, the authors are less interested in providing policy prescriptions than in seeking an explanation of how West Germany's national interests have been integrated into broader transnational security and economic interests. In the essay on the country's management of security relations, Helga Haftendorn provides a most useful account of the complex decision-making process, noting that domestic political forces, except for organized industry, have had limited interest in or influence on the country's security policies. As a consequence, the bureaucracies and experts have had considerable freedom to recommend or shape policies.

In another essay, Joachim Krause and Lothar Wilker deal more specifically with the bureaucracy's role in shaping foreign policy. They concur with Haftendorf's thesis, but also point out that low-level civil servants can pre-select political alternatives, impede political changes, and prevent the development of comprehensive concepts—even when political leaders draft guidelines and manage crises. Yet these civil servants also must work within the parameter of international economic developments and the interests of Germany's alliance partners.

One major essay, by Frieder Schlupp, concerns Germany's key role in the international political economy. Schlupp argues that the country's economic strength is a mixed blessing at best, because it forces other West European countries into a pattern of instability and dependence, thereby making West Germany an "international villain" in their eyes. These countries also express concern with the key position of the Federal Republic in United States-European relations and in the trilateral world, and resent West Germany's implicit claim that its policy of stabilization is the model that they ought to emulate.

The author also emphasizes the role the Federal Republic plays in the inter-capitalist economy: in international trade and foreign investment, and in the internationalization of its capital. Giant German high-technology firms have captured an increasing share of the world's market, although foreign multinational companies have made inroads into the domestic market. He calls for a "structural transformation of the very economic base, and political superstructure of the "Modell Deutschland' into a societal emancipation and political autonomy" based on a democratic-socialist system "beyond the systemic deformations of both late capitalism and 'real socialism' " (pp. 91-92).

Three essays deal with the Federal Republic's policies toward the German Democratic Republic, the East, and the Third World. Gebhard Schweigler argues that in both Germanys a strong national consciousness is developing, replacing an all-German state consciousness, and killing the hope for an eventual reunification. Michael Kreile briefly traces the foreign and domestic implications of West Germany's Ostpolitik. He notes that improved relations with the Easst Bloc can only be achieved by a detente policy vigorously supported by the United States and Western Europe. Ulrich Albert argues that Bonn has not been sensitive enough to the legitimate aspirations of Third World countries because industrial penetration and Western-style tutelage continue the pattern of former colonial relations, especially in Africa where Germany's interests have not flagged over the decades.

Ekkehart Krippendorff has written a brief introduction to this volume, which, however, as so many other multi-authored works, suffers from a lack of comprehensive and balanced treatment. For instance, the subtitle promises a coverage of the contents of foreign policy, but the reader will find little on Germany's relations with Western Europe, the Atlantic Community and China.

While there are gaps, a lengthy chapter is included on the development of international relations studies in West Germany (Gisela Gantzel-Kress and Klaus Juergen Gantzel) that only has tangential relation to the theme of the volume, although it will be of interest to political scientists. As useful is a bibliography of West German political science literature for 1977-1979 (compiled by Volker Rittberger). Despite these problems of coverage, the radical critique of West German foreign policy found in most essays is path-breaking and a useful corrective to the more establishment-oriented volumes on the same topic.

GERARD BRAUNTHAL

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Governing Trade Unions in Sweden. By Leif Lewin. Translated by Kjersti Board. (Cambridge, Mass.: Harvard University Press, 1980. Pp. v + 180. $20.00.)

In this book, Leif Lewin reports research designed to test whether the Swedish trade union movement (LO) is governed through "interactive democracy." The data are 1974 mail survey results from a sample of approximately 2300 members and 650 officers drawn from 50 locals, plus 250 central officers. The research was funded by the Bank of Sweden Tercentenary Fund.

Lewin devises "interactive democracy" as a way out of Michels' dilemma. Lewin writes that "oligarchy within democratic organizations can be avoided if the interaction between leadership and membership functions in such a way as to develop the public spirit of the individual" (p. 16; *viz.* J. S. Mill). On page 54 Lewin reveals that public-spiritedness is membership support for LO's official wage policy of solidarity.

Interactive democracy requires both political participation by the members and "creation of opinion by the leadership" (p. 20). It is not enough, or even desirable, that leaders perfectly reflect their constituents' preferences. Elites should "in some cases be required to go against public opinion" (p. 19). Further, "competition [among elites] cannot be viewed as a sufficient condition for democracy, and may not even constitute a necessary condition. . ." (p. 25).

Since Lewin sampled first the 50 locals and then members and officers within each local, he is able to score each local on the three dimensions he says are critical to interactive democracy: *opinion formation* (member and leader activity, competence of members and leaders, and "system orientation"), *consensus* of members and leaders on union priorities and values, and *authority* of leaders (ability to take action without protest).

Locals which satisfy criterion values for opinion formation, consensus, and authority are "democratic" (26 percent of the sample). Locals below each of the three criterion values are "passive" (30 percent). "Impotent" locals (14 percent) lack authority, "manipulative" locals (14 percent) lack opinion formation, and "therapeutic" locals (16 percent) lack consensus.

Public spirit of each local is scored by the members' responses to three questions supposedly probing the members' internalization of the wage policy of solidarity. "Democratic" locals score either high or medium on public spirit whereas about half of the "passive" locals scores low. (It is also true that half of the "passive" locals scores medium or high.) The conclusion is that "democratic" locals generate public spirit.

This study is so badly flawed that its conclusions must be rejected.

Cross-sectional data are inappropriate for testing a model of member-leader interaction. Lewin asks members and leaders about their generalized activity levels in 1974 and relates this activity to support for a norm that has been official LO policy for 20 years. Case studies providing process data, rather than cross-sectional survey data, are necessary to evaluate Lewin's model of interactive democracy.

Lewin relies on reported activity even where behavioral data are available. Lewin reports that " 'voting in union elections' is not indicated as a measure of activity, because . . . the claim of voting participation was so exaggerated that the measure proved useless. . ." (p. 173). Lewin's peculiar solutuion is to ignore the actual voting data and rely instead on member reports on how often they attend meetings and whether they believe in worker solidarity. In another case, however, Lewin has it both ways. He reports that about 5 percent of LO membership participates in study circles (p. 48) but accepts without comment that 11 percent of his representative sample reports participation in study circles (p. 78). Lewin opts for unreliable survey data when hard bureaucratic data are available.

The operational meaning of the dependent variable, public-spiritedness, is garbled. Responses to three different interview questions are added together to give each local a public spirit score. Seventy-eight percent of the members agree with the normative statement that higher-paid groups should support low-income earners even if this means that their own demands are set aside. Only 47 percent, however, say that they personally would forego a raise to benefit low-income workers in other unions. The third question is whether "it is wrong of LO to make such wage demands that the least efficient industries are forced out of business" (p. 76). Forty percent disagree. It is not obvious that these questions are equivalent. No intercorrelations are provided to allow the reader to assess whether the questions tap different dimensions, in which case an averaged score is meaningless.

There is no independent measure of public spirit. Consensus, an independent variable, is constructed from, *inter alia,* the same questins used to score public-spiritedness. It is not surprising that 63 percent of the "therapeutic" locals (which lack only consensus—"therapeutic" locals have both opinion formation and authority) have low public spirit. On the other hand, of the 27 locals "having" at least consensus, only 2 have low public spirit.

Authority, one of the three constructed variables explaining public spirit, is conceptually vague and operationally useless. We are told on p. 27 that "union leadership enjoys authority if it is able to act upon the decisions made without encountering protests from the members," which is quite different from the statement on the following page, "Authority signifies that union policy is approved because the leadership has convincingly argued its case."

For Lewin, the *local* leadership's authority is the percentage of its members which both does not agree that "the union managed to derive maximum benefits from the latest [centrally] negotiated agreement" and claimed *not to have noticed"* wildcat strikes or protests at their workplace (p. 108, emphasis added). This leads to the peculiar observation that in one local, 8 percent of the workers noticed wildcat activity while the remaining 92 percent, presumably at the same workplace, did not (p. 124). Lewin accepts the 8 percent as the actual extent of wildcat activity (pp. 109, 124).

Calculations of local scores for the other two independent variables, opinion formation and consensus, are mindless number crunching. Opinion formation, for example, is constructed from 31 separate questions, each assigned a different numerical value. The most elementary rules for index construction are ignored. The analysis is replete with double counting, averaging apples, oranges and potatoes, and mistaking horses for zebras. Whatever opinion formation may be conceptually, it cannot emerge with any validity from this construction. If, for example, opinion formation in a model of interactive democracy is supposed to reflect membership activity, it must be conceded that membership activity items account for only about one-ninth of the summary score (pp. 116, 122). Having "attended a union meeting three times or more," "contributed to discussion," and "discussed union matters with a committee member" together are worth as many points as being able to name the union president (p. 116). Also, the sample calculation of the opinion formation score on page 122 is incorrect.

Lewin on page 95 concludes that "competition does not further legitimacy" after finding that 75 percent of the members of locals with competitive elections are critical of union elections (versus 72 percent of the members in locals without competitive electins). Nearly all of the members' criticisms of union elections can as easily be dissatisfaction with the election results as challenges to their legitimacy (p. 92). It may be that the problem with competitive elections is that someone loses.

This book should not have been translated. Specialists are already familiar with the book in Swedish. It is a pity that other readers will learn very little about the Swedish labor movement from the book, and much of that will be misleading. Still, Kjersti Board deserves credit for a true and readable translation.

DON S. SCHWERIN

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Arabs in the Jewish State: Israel's Control of a National Minority. By Ian Lustick. (Austin: University of Texas Press, 1980. Pp. xii + 385. $19.95, cloth; $10.95, paper.)

Arabs in Israel have been a vastly neglected group in the large body of literature on Israel and its political life. Ian Lustick's study is the first major, systematic analysis by a political scientist of Israel's policies and practices toward the 15 percent of Israeli citizens who are Arabs. It brilliantly focuses on the causes behind the low level of political activity on the part of Arab citizens of a country surrounded by hostile Arab neighbors, with important internal political divisions, with substantial grievances on the part of its indigenous Arab population on issues such as land expropriation, and unequal educational and occupational opportunities. Though formally possessing full civil rights, Arabs in Israel neither have an independent political base nor have they been able to establish a mass-based civil rights movement. They control no independent newspaper and in effect possess no significant leverage in the political process.

Lustick argues convincingly that this political quiescence does not derive from the failure of an inept traditional leadership to use effectively the resources of the political system to extract concessions and derive greater benefits. Quite the contrary, Lustick's thesis is that "the failure of Israel's Arab minority to 'organize itself' and the minimal significance, to date, of the communal segmentation of Israeli society of the operation and stability of the Israeli political system are due to the presence of a highly effective system of control which, since 1948, has operated over Israeli Arabs" (p. 25). The purpose of the book is not to decry this system but to describe how it has worked to serve the goals of a regime which serves the interests of the dominant Jewish population; to explain why the strategies used have been so successful, notwithstanding the considerable resentment and potential resistance of the Arab minority, and to consider the possibilities of changes in that system in the years ahead.

Lustick presents a history of the Zionist movement prior to 1948 in terms of how it dealt with the question of the role of the non-Jewish minority in the posited Jewish state, and how the new government responded to the problem of the Arab community that remained within Israel's borders after independence. The imperatives of that new relationship centered on five major concerns for the Jewish state: security, territorial consolidation, immigrant absorption, economic growth, and political stability. In each of these areas Israeli policy was fueled by the primary goal of controlling the Arab minority rather than integrating, absorbing, developing or eliminating it.

The analytic framework for the comprehensive examination of the control system focuses on three levels of analysis—structural, institutional and programmatic. Three components of control —segmentation, dependence and cooptation are identified and described in separate chapters.

Segmentation was implemented through the encouragement of structural fragmentation of the Arab minority along religious and clan lines, through demographic isolation in population enclaves, and via the nearly 20-year military government over the Arab sector, through a policy of unequal access to the resources of the country. Lustick concludes that segmentation served to deprive Arabs in Israel of "facilities for united political action, whether involving alliances among Arabs on a country-wide basis or between groups of Arabs and politically significant groups of Jews" (p. 149).

Dependence as a component of control is analyzed in terms of the sustained, massive and unavoidable reliance of Israeli Arabs on the Jewish sector for education, work permits, status, and all the essentials of economic, social and political resources. The Arab population totally lacks a viable, autonomous basis of economic power, not fortuitously, but by regime intent. This has made them, not surprisingly, quite vulnerable to noncoercive forms of economic pressure and reprisal.

Lustick suggests that while segmentation and dependence have become institutionalized, *cooptation* of Arab elites has not. Rather, those who are subject to cooptation—sheiks, school administrators, scoutmasters, and others—have been used in a highly instrumental and summary fashion. Cooptation is based on material inducements, threats of material deprivation, and the claims of self-interest, rather than any effort to socialize coopted elements into the dominant ideology or bring them into the mainstream of Israeli life. It is quite clear that for the Arab population in Israel, the doors of advancement are structurally limited throughout the society. Cooptation serves mainly to capture the existing Arab elite and to weaken the incipient forms of leadership that might otherwise spearhead articulation of needs and interests of their potential constituency.

In the concluding chapter, Lustick interprets the synergistic relationships among the three factors. He finds that the system of control has been very successful in achieving the regime's goals at minimal costs of resources and effort. He predicts that although there are not likely to be any massive changes in the near future, the regime is likely to have to put more effort into the control system in order to maintain its position. He believes that Israel's governing elements will eventually have to decide between continuing the prevailing control system (the Toledano-Histadrut line) or opting for the "tough," repressive policy position (the Linn-Sharon line). In either case, he thinks, costs are going to escalate and in the long run the quiescent state of the Arab community will not continue. Internal developments and changes in the external environment will bring about a significant shift in Arab political activism.

One of the great virtues of this work is that it avoids the academic shoals of apologia on the one hand, and of radical polemics on the other. Lustick's study weakens the argument of the majority of writers on the subject, that the Arabs in Israel are a modernizing and adjustable minority in a Western society; that the new state has significantly improved the overall position of its Arab citizens, and made an honest effort to make them equal before the law. At the same time, it eschews the simplicities of those who see Israeli Arabs as part of an oppressed Palestinian national minority in an exploitative and "racist" Zionist state. Lustick uses a conflict perspective in his analysis and presents Arabs in Israel as a vulnerable, controlled, nonassimilating and underdeveloped national minority in the Western, Jewish, Zionist state.

This comprehensive study is meticulously researched, cogently argued and lucidly presented. Lustick builds his argument on the extensive use of government documents, Hebrew newspapers, Zionist archives, and interviews with prominent Arabs and Jewish officials in government, political parties, labor unions, and non-governmental Jewish agencies. He is commendably careful not to sit in judgment. The book, though very well written, possesses an element of redundancy that may grate on those familiar with the subject. Another minor flaw is the inadequate attention paid to some of the more "hidden costs" of the Jewish state's control policies for both Arab and Jewish citizens in terms of morale, personal and national identity, social consciousness and sense of justice.

This study will not please those who see Israel as the unblemished bastion of democracy in the Middle East, nor those who are persuaded that it has treated its Arab minority in an exemplary fashion. Though this book may provide ammunition for the critics of Israel, at some point it must be said that there is considerable value in having this thorny terrain examined in a calm, scholarly and dispassionate fashion.

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The Making of a Stormtrooper. By Peter H. Merkl. (Princeton, N.J.: Princeton University Press, 1980. Pp. 328. $16.50.)

The Hitler Movement: A Modern Millenarian Revolution. By James M. Rhodes. (Stanford, Calif.: Hoover Institution Press, 1980. Pp. 253. $14.95.)

The two books reviewed here are very different in both theme and methodology, but they are part of a by-now voluminous yet still growing genre: the literature on why and how the Nazis and Nazism. Both Merkl and Rhodes accept implicitly that the Third Reich remains a historical phenomenon that is, if not *sui generis,* of far greater import than the mere substitution of one group of politicians for another, as Lord Rothermere once put it. In addition, the two works also share parallel documentary bases. Both are largely synthetic accounts, relying heavily on reinterpreting known literature and sources, but Peter Merkl and James Rhodes do make use of one major archival collection, the so-called Abel collection at the Hoover Institute at Stanford University. As a historical source, this group of documents has some problems (about which more in a moment), but this is not to deny its usefulness for both books.

Rhodes takes what might be called a theophilosophical approach. He seeks to find a proper ideological categorization of Nazism and the Nazis, and is doing so to explain both motivation and socio-political success of the Nazis. These questions, as Rhodes readily admits, are hardly new, but he feels that all previous causal explanations of the phenomenon (the societal breakdown effect of World War I, the lower-middle-class thesis, the German national character fault, etc.) have been inadequate or contradictory. Merkl's aim is somewhat more modest. He is concerned with the motivational forces and internal categorizations of the largest and most violent segment of the NSDAP during its movement phase, the storm troopers (SA). These politicized paramilitary units played a major part in bringing Hitler to power, though they were quickly eclipsed in the power struggle with Himmler's SS after 1933. Like Rhodes, Merkl is not satisfied with the previous explanations of the reasons that led to membership and activism in the SA, and he attempts by a variety of sophisticated quantitative and qualitative methods to delineate different chronological segments in the SA's membership and to correlate these age groups with prevalent types of activity (from primarily poster-pasting to a near-exclusive concern with political brawling).

Methodologically, both authors attempt to rest their thesis upon a combination of macro-documentary bases and the use of direct, personal testimony of rank-and-file Nazis. For the former, Rhodes uses philosophical and theological sources reaching back to the visions of John in the *Book of Revelations* of the New Testament; Merkl makes abundant use of the literature on the Weimar Republic and on political motivation in mass movements. For their micro-sources, both authors rely heavily on the Abel collection. At this point it might be useful to express a few caveats about this collection of documents. It consists of 581 testimonials (originally 681, but 100 were removed from the collection by the FBI in the early 1950s and have been lost since) of NSDAP members regarding their reasons for becoming Nazis and their activities as party or SA members before 1933. The statements were solicited (under the pretext of a contest for the best essay) shortly after the Nazis came to power by the sociologist Theodore Abel, who first used part of the information in his book, *Why Hitler Came to Power* (1938), reprinted under the title, *The Nazi Movement* (Atherton, 1965). Merkl has subsequently made much more extensive and sohisticated use of the materials in his work *Political Violence under the Swastika* (Princeton, 1975). The Abel documents have the advantage of being a primary source written shortly after the events described, and they are the expressions of genuine rank-and-file Nazis, rather than pronouncements by the leadership. (Few of the "Abel-Nazis" achieved any prominence in the Third Reich.) On the negative side, they are heavily weighted in favor of pre-1930 members, geographically unevenly distributed, written in the immediate flush of political victory before the *Katzenjammer* that set in by 1935, and there is of course no means of verifying the factual statements. As I have noted previously (*Societies,* 8 [1978], 71-73) there is considerable evidence of factual and motivational embroidering. Incidentally, it would be most interesting to compare the statements submitted to Abel with the same individuals' account of their lives given in the course of their de-Nazification proceedings after the war.

Both authors present easily comprehensible, vigorously defended theses. Rhodes contends that the Nazis were essentially a secular millenarian movement, and, except for the adjective "secular," comparable to any number of previous attempts to create utopian societies on earth. Rhodes takes a somewhat unique approach in that he sees millenarian movements not merely as socio-historical phenomenon, but as forms of sin and heresy directed against a divinely ordained, Christian-oriented societal order and morality. In supporting his thesis, Rhodes cites numerous millenarian authors beginning with John of *Revelation* to establish the characteristics of millenarianism. He then uses statement from Nazi propaganda and the Abel collection to illustrate the specific Nazi forms of this disorder.

It is difficult to "review" Rhodes' thesis, since he argues outside the established conceptual framework of the social sciences. He proceeds from an essentially theological and teleological standpoint which holds that not only factual statements are verifiable, but moral and political beliefs are objectively right or wrong as well, because within their sphere divine law sets absolute standards. Thus as millenarians the Nazis were wrong because they should have known that all millenarianisms are Christian heresies. Rhodes points out, for example, that those Germans who expected the pre-World War I prosperity to continue were not just naive, but objectively wrong in their belief: genuine fulfillment can never be achieved in this life, but only after death in the presence of God. Any other expectation is a hope for false salvation and therefore heretical.

While Rhodes' reasons for condemning millenarians are largely matters of faith and thus not subject to the usual tests of social scientific verification, it is possible to examine the methodological path he travels to reach conclusions, and here a number of major questions seem appropriate. Rhodes returns to viewing Nazis and Nazism as a monolithic bloc undifferentiated by either factions or chronological segments. This view, it seems to me, empirical research has long since disproved. Moreover, in order to cover all observed characteristics of Nazism with the millenarian umbrella, Rhodes is forced to enlarge the concept almost beyond recognition. If, for example, calling for an end to corruption in public life or worship of military heroes are typically millenarian sentiments, then clearly all politicians in a modern mass context are millenarians. In essence, Rhodes' book, while laudable in its aim to condemn Nazism as immoral and sinful, suffers from too many questionable conceptual and methodological assumptions to become a major contribution to the historical literature on Nazism.

Merkl's work, on the other hand, fits into the mainstream of social science methodology and conceptualization. The author shows that far from being a monolithic body of ideologically committed men, the Nazi storm troopers were composed of three quite distinct age groupings: those old enough to have served in World War I, a second group too young to have fought though they did experience the war consciously ("the victory watchers"), and a third cohort composed of those who were essentially still children before 1918. In addition, Merkl is able to correlate political attitudes and activities with each of the age groupings. This does not mean, however, that the SA members as a whole do not show some distinguishing characteristics: as a group they can be differentiated from the party membership in terms of both activity and motivation, and the primary motivational force for joining the storm troopers was not economic hardship or ideological conviction, but a sense of alienation resulting partly from the *Zeitgeist* of the Weimar Republic but more significantly from personality problems of the individuals. The SA members, in other words, were misfits, and Nazism gave their asocial tendencies the sanction of political sacrifice and national service. The extraordinarily large number of such misfits among that generation of Germans Merkl attributes to a series of national shocks that confronted this generation of Germans; the defeat in World War I, the post-war Freikorps operations the chauvinistic reaction to the Ruhr occupation—all this led to a very high degree of violence and para-militarization of Weimar politics.

It is difficult to quarrel with Merkl's overall conclusions; his thesis is empirically well supported. The difficulties with using the Abel collection remain, but unlike Rhodes, Merkl is clearly aware of the caveats and tries to compensate for them. I do think Merkl may be painting the paramilitary background of Weimar politics in too stark colors. It might be argued that viewing all 15 years of the republic as a unit is historically unwarranted as is lumping the SA and the Reichsbanner Schwarz-Rot-Gold into the same category of para-military organizations. Membership and motivation of the two groups was really quite dissimilar. A few minor items of style and formulation are jarring. The description "Prussian-ruled Poznan" for the province of Posen in 1914 seems to give the social scientist the job of retroactive political rectification. After all, Posen at that time had been part of Prussia since the 1780s, and by that standard South Dakota could still be styled "American-ruled Sioux territory."

But these are minor items of criticism in an otherwise important addition to our knowledge of Nazism. Not least among Merkl's contributions is to disprove Rhodes' thesis. The storm troopers were clearly not a bloc of millenarians, but variegated social misfits who joined the SA for a number of reasons, among which the desire to engage in physical violence was prominent. Merkl's book demonstrates yet again that attempts to explain Nazism and Fascism in terms of mono-causality, including that of Rhodes, remain unconvincing.

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Palestinian Society and Politics. Edited by Joel S. Migdal. (Princeton, N.J.: Princeton University Press, 1980. Pp. xv + 290. $20.00, cloth; $6.95, paper.)

Multi-authored volumes tend to suffer from substantial gaps, even when the editor provides an overview and commentary to bridge the individual essays. Nevertheless, such volumes can usefully collect related writings that would otherwise be scattered across different journals or available only in dissertation form. This is true of the study under review, since many of the essays are based on primary documents from relatively inaccessible Israeli archives and several are excerpts from unpublished Ph.D. dissertations.

Palestinian Society and Politics contains an extended essay by Joel S. Migdal and eight chapters by Israeli and American scholars. The volume seeks to cover a broad range of issues, but actually deals with a limited number of themes and a constricted geographical context. The analyses of the pre-1948 period take the entire mandatory territory as the geographical unit, but those covering the post-1948 period concentrate virtually exclusively on the West Bank. They thereby omit the majority of the Palestinians, including those who experienced the most severe disruption in 1948 and who now lead the Palestinian political movement.

Moreover, the essays are primarily concerned with village society. While serving as a useful corrective to studies that emphasize elite politics and ignore the lives of the peasants, they tend to overstate the importance of villagers in Palestinian political life. The study thus contains several interesting but esoteric micro-analyses on the village mukhtar (Gabriel Baer), "The Impact of British Norms on Arab Community Life" (Ylana N. Miller), efforts to protect cultivators in Mandatory Palestine (Kenneth W. Stein), internal labor migration in the 1940s (Rachelle Taqqu), and a comparison by Migdal of social cleavages in 10 West Bank villages in 1973.

Although the impact of British politics on the peasantry is delineated, the essays tend to understate the impact of the Jewish presence on the Arab community and the inability of the Arab sector to compete with the Zionist movement economically as well as politically. Nonetheless, Migdal does comment that the presence of the British rulers and the Jewish community impeded the unfettered development of a large, strong Arab middle class. Miller contrasts the impoverished Arab villages of the 1930s with the effectively organized Jewish community, and Taqqu notes the wage disparities between Jewish and Arab laborers in the 1940s. Moreover, Stein presents an intriguing but incomplete analysis of the symbiosis between the Jewish land-purchaser and the Arab land-seller who frequently colluded to circumvent British legal measures intended to protect tenants against eviction.

Several essays point out the contradiction between the aims of ruling regimes and the actual outcome of their policies. Migdal argues that the British allied with the urban, landowning elite but pursued policies which weakened that elite's bases of power and accelerated the fragmentation of village society. Jordan was more successful in co-opting the traditional elite into the state apparatus. While Israeli military rule sought to weaken both the traditional elite and the radical intellectual counter-elite and to preserve only local administrative structures, its economic policies inadvertently caused substantial social change. Baer supports this argument by describing the contradictory effects of increasing administrative and security demands on the village headman. Taqqu also depicts the unpredicted social impact of mobilizing village workers during World War II, and Shaul Mishal details Jordan's methods of simultaneously co-opting and fragmenting the Palestinians on the West Bank. Mark Heller traces Israeli policy since 1967: by denying a mediating role to the Jordanian-backed notables and refusing to let any representative framework be established on the West Bank, that policy resulted in not only the radical counter-elite but also the traditional elite actively opposing the occupation.

In analyzing current Palestinian politics, Heller asserts that neither Israel nor Jordan can authoritatively allocate political values on the West Bank, and only the Palestine Liberation Organization can effectively control the Palestinians' political and moral resources. This argument is reinforced in the essays by Shimon Shamir and Donna Robinson Divine. Shamir, interviewing refugees in a West Bank camp in 1968, found Palestinian identity to be the dominant value and Jordanian identity the weakest. Divine, observing more broadly the problems created by the absence of political independence, argues that subjugated societies contain no paramount political power center around which the system pivots. The PLO represents a significant departure from previous Palestinian political organizations, she maintains, in that it has reconstituted a national elite and a political center, even though social cleavages and geographical dispersion still persist and contribute to limiting the PLO's effectiveness.

Migdal has assembled a provocative collection of essays which can be used effectively by the researcher who already has an overview of Palestinian history and knows how to interpret and integrate these diverse selections.

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Values, Identities, and National Integration: Empirical Research in Africa. Edited by John N. Paden. (Evanston, Ill.: Northwestern University Press, 1980. Pp. viii + 397. $37.95.)

This book is a reflection of things past and, perhaps if we are lucky, a harbinger of things to come. Ably edited by John Paden, this collection relates research undertaken on national integration in Ghana, Ivory Coast, Kenya, The Gambia, Nigeria, Cameroon, Liberia, and Zaire. A product of the National Unity Project of Northwestern University, this volume contains, for the most part, essays drawn from the doctoral dissertations of students in sociology and political science. It is thus very much an "in-house" product.

Paden's introductory chapter both summarizes the findings of each contributor and surveys the relevant theoretical underpinnings (systems theory) which inform the work. Values and identities are identified as key components of national integration and defined. Values are seen as "modal patterns of preference which characterize broad segments of a population" and identities as "the ingroup and outgroup ascriptions of 'labels' or 'names' to aggregations of people which may have social, political, or economic relevance" (p.

5). The key question, asked by each of the researchers in his or her own style, is "what are the patterns of values and identities, especially at a 'mass' level, which affect orientations toward authority, community, conflict resolution, and innovation?" (pp. 3-4). With this as a common starting point, the authors propose to examine the relationships between values, identities, and national integration; to explore the notion of ethnicity through its empirical manifestations; and to provide data for other cross-national studies which often omit Africa from their purview (p. 5). Although there was no formal, common research design, the contributors had extensive contact with each other at all phases of the project and the final product reflects this. Unlike many edited collections, this one does hang together and the authors successfully address a common theme.

This is not to say, however, that all of the contributions are of equal quality. Joseph Kaufert's work on situational ethnicity among university students in Ghana is particularly noteworthy. He specifies population density, mobility, heterogeneity, disproportional representation, and economic differentiation (p. 56) as key variables which interact, in different circumstances and contexts, to produce identities based on kinship, town of origin, language groupings, regional origins, and nationality. His research is both subtle and informative and is one of the few attempts to provide a systematic categorization of possible ethnic identity outcomes. Kaufert recognizes, correctly, that much more effort needs to be put into an inventory of situations. His study is merely a starting point, but a very good one.

The chapters on ethnicity in Cameroon by Ndiva Kofele-Kale are also worthy of sustained attention. In a compelling exploration of political culture in English-speaking Cameroon, the author creatively weaves quantitative data with extracts from unstructured interviews. Confident in his use of both methodologies, he is a prisoner of neither. (Regrettably, not all of the contributors to this volume are as skillful.) Kofele-Kale attempts to measure patterns of authority in family and ethnic group and his results deserve a wide hearing. He finds for example, that "rural ethnic man and urban ethnic man are essentially the same with regard to exposure and attachments to ethnic group values" (p. 149) and that loyalty to nation, on the one hand, and ethnic group on the other, are not necessarily mutually exclusive (p. 170). These essays on Cameroon also stand out because they consider certain social class considerations and note the differences in perceptions of government among urban, semi-urban, and rural people. Those in the countryside have a much more negative view of government largely because they have been the victims of an increasing gap between rich and poor (p. 154). Kofele-Kale has furnished us with two methodologically creative, intellectually rigorous, and theoretically relevant essays.

Amos Sawyer's chapters on Liberia are also recommended for they are the only ones in this volume to deal explicitly with social stratification. They are all the more important in light of the recent military coup and overthrow of the dominant Americo-Liberians. His conclusion that "the effective utilization of the institutions of the political system in the interests of the people of the system, and the fair and equitable distribution of the goods and services of the system play no small part in instilling in the population the value predispositions essential for national development" (p. 320) might once have been read profitably by President Tolbert.

Despite the generally high quality of these essays and the occasional outstanding research reports cited above, I concur with Ronald Cohen's epilogue to this volume. Unfortunately, the "inductive behavioral procedures did not lead to major breakthroughs in macrotheory" (p. 326). The question then becomes one of future research orientation. Paden's conclusion argues that replication is necessary not only to check reliability, but more importantly to provide the time-series data so crucial for the comprehension of any social change (p. 359). While this position makes sense, it is simply not enough. Future work must not only replicate what has gone before, but should also expand into new realms. Cohen indicates that forthcoming studies of internal processes must also consider the external factors pinpointed by theorists of dependency. He further argues that while ethnicity and regionalism are likely to remain salient axes of political conflict in Africa, we should also pay attention to the rural-urban continuum and social class. It is in the failure of most of these authors to treat social class as a component of national integration that I find myself most at odds with this volume. Their work, with some exceptions, is important and well done. It is, however, too narrow. Cohen's thoughtful essay on the future of socio-political research in Africa should be read by all working in this field.

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Democristiani. Edited by Arturo Parisi. (Bologna: Societa editrice il Mulino, 1979. Pp. 231. Lire 6,000.)

This volume gathers efforts of scholars well-versed on the Italian Christian Democrats. In the aggregate, their essays bring us abreast of important changes, especially at the level of orientations, occurring within the DC community as it attempts to cope with new, oftentimes unfavorable, changes in its socio-political environment. Among the matters considered are DC member attitudes towards their own party, the Catholic world, and their main adversary, the Italian Communist party. The essays offer the reader a rich body of current data which is insightfully interpreted. Overall, the DC is portrayed as undergoing, somewhat reluctantly, an evolutionary process that is altering its character as a party and its role in the Italian political system. A careful reading of the *Democristiani* will provide ample rewards. Brief glimpses of its contents follow.

The essay by Maurizio Rossi charts the rise, fluctuations and partial decline of the membership of the Christian Democratic party. In response to the organizational challenge of the Italian Communists, and anxious to reduce its political dependency on the associations of the Catholic Church, the DC leadership recruited more than 1½ million members during the late 1940s and early 1950s. Thus, and in spite of the perennial inflation of the DC's official figures, there can be little doubt about the mass-based character of Italy's dominant party. At the same time, in the view of Rossi, the DC never fully achieved its aspiration to be an "inter-classist" party: it remains basically a middle-class party—notwithstanding its efforts (partially successful) at broadening its social base of member recruitment.

The overall success of the DC in mobilizing a mass membership has not, unfortunately, been accompanied by similar success in meaningfully involving its members in the internal life of the party. Indeed, in the considered judgment of Douglas Wertman, the attitudes and comportment of the DC's leadership have tended to discourage the party's rank-and-file from assuming an effective and meaningful role in the party structure. The lack of responsiveness of national leaders of the party to the activity of members at the local levels has undermined its value, leading ordinary members to conclude that their participation in the party is inefficacious. This negative perception then blunts their interest in and commitment to the party. When other factors are added—the indirect recruitment and hence weak ties of women, the retarding effect of clientelism, and cultural norms which discourage female political activism—the level of participation in the DC is further reduced.

After mapping out the size, social composition and participatory profile of the DC's membership, the inquiry turns to the relationship between party members and organized Catholicism. Using attitudinal and behavioral indicators, Arturo Parisi concludes that the DC is still a "party of Catholics"; e.g., 53 percent of the members are "regular practitioners," a quota triple that of the general population, greater than that of any of the other parties, and far in excess of the quota for PCI members. Moreover, Catholic militants who are politically active and belong to a party are overwhelmingly Christian Democrat. Parisi notes, however, that important changes are occurring among younger generations: they are becoming less "Catholic" and less accepting of the DC. To survive politically, Parisi concludes, the DC is relying less upon its appeal to Catholics and more on its "catch-all" character.

Some corroboration of this trend is found in the essay by Piero Ignazi and Angelo Panebianco. Their data indicate that the confessional perspective among DC members is definitely on the wane. The clerical right ("Traditionals") and clerical left ("Integralists") account for only one-fifth of the party's membership. The lay or secular perspective is now shared by a preponderant majority of the party, half of whom are characterized by Ignazi and Panebianco as "left" (prefer political formulas which include the PCI; feel "close" to parties of the left), and the other half as "right" (favor PCI exclusion and feel antipathy towards the left). The members of the two groups—the "Democratics" (lay-left) and "Conservatives" (lay-right)—are mainly well-educated, enjoy high professional status, do not observe religious practice, and are younger than the clericals of the party. Furthermore, their numerical strength and personal and social resources assure them a place of power in both the party and in the elective/administrative arena, from which the clericals have been largely displaced. But although the religious ties of the bulk of Christian Democrats have been weakened, so long as the Conservatives remain strong, the posture of the DC towards Catholic interests, as well as towards the PCI, will change only slowly.

But change it will—especially with respect to the Italian Communists, as Gianfranco Pasquino ably demonstrates. In fact, sizeable minorities now favor (41 percent) and/or predict (46 percent) DC coalitions with the PCI (with the latter preferably in a subordinate role). This does not imply that Christian Democrats have found new sympathies for their old adversary; it is, rather, a manifestation of the political realism and operational pragmatism which has for so long characterized Christian Democrats and has contributed to their success, over the course of more than three decades, in maintaining a position of dominance in the Italian political system.

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Mobilità senza movimento: Le elezioni del 3 guigno 1979. Edited by Arturo Parisi. (Bologna: Societa editrice il Mulino, 1980. Pp. 160. Lire 8,000, paper.)

The title of this book accurately captures the dilemma its contributors face. After five years of what appeared to be considerable movement on the part of the Italian electorate, the 1979 general elections suggested, at first glance, a shift away from the fluidity of the seventies. In spite of this apparent change, and despite numerous differences of interpretation within the present collection, all five authors agree that we are not witnessing a return to older patterns. Something changed in 1974, and the absence of dramatic movement in this particular election does not indicate an immobile or static electorate in Italy.

This central proposition is easy to accept, especially since the contributors offer qualified arguments and numerous caveats concerning the various measures they employ to analyze recent voting patterns. Moreover, aside from its own considerable merits, this volume is welcome as part of a much larger and longer-term undertaking, supported by the Istituto Carlo Cattaneo, which should provide a comprehensive overview of recent Italian electoral behavior.

Arturo Parisi's introductory essay gives coherence to the collection. He furnishes a useful synthesis of the proliferating interpretations (many of them mutually contradictory) of recent electoral fluctuations, and offers a number of highly persuasive arguments to support the core thesis spelled out above. He admits that no definitive conclusions can yet be drawn, and emphasizes the need to integrate different types of analysis into studies of voting behavior. Especially important, in light of recent Italian political history, is the emphasis he puts on analyzing the parties' activities in a period of increased voter mobility.

In view of Parisi's admonition, it is a pity that only Gianfranco Pasquino's analysis of the Socialist party fully integrates explicitly strategic and political issues into the broader electoral analysis. Like the others, Pasquino shows how the vote has a distinctive logic in different areas of the country, a logic often washed out by correlation coefficients applied only to the national level. But, in an essay rich in suggestions and interpretations, Pasquino is also able to demonstrate the interaction between the variegated vote and the behavior and intentions of the Socialist leadership.

The big loser in 1979 was the Communist party, and Renato Mannheimer's territorial analysis of its losses helps put them into perspective. Here again, the importance of different voting patterns in different areas of Italy is very effectively underlined. In some instances, national patterns are clearly discernible—e.g., the PCI lost most in 1979 where, in 1978, the referendum on the public financing of parties showed the greatest resentment against the party system. But even more frequently, different patterns emerge: the drop in voter turnout hurt the PCI in southern Italy, but not elsewhere; the Radicals were able to capture disaffected Communist voters in central Italy, and especially in the north, but not elsewhere.

The other contributors also emphasize the regional specificity of the vote in most instances. Piergiorgio Corbetta's study of three north-central cities is the most restricted in scope, but it has the advantage of being able to focus on changes at the level of the "ward" (*senzione elettorale*) over time. Corbetta also is most explicit in arguing that changes initiated in the mid-seventies continue apace, demonstrating that what appears as stability is often revealed on closer scrutiny as extensive self-canceling movement between the parties. Maurizio Rossi has the unenviable task of trying to summarize the fortunes of the entire center-right of the political spectrum, including of course the dominant Christian Democratic party. The result, imposed by the subject matter, is diffuse, while confirming the multi-directional movement of the 1979 vote.

As a study which claims to be no more than a contribution to an ongoing analysis and debate, this book is quite successful. It is necessarily tentative in view of the evidence thus far at hand, but it raises the important questions and points to the problems that will have to receive more attention in the future.

STEPHEN HELLMAN

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The Apartheid Regime: Political Power and Racial Domination. Edited by Robert M. Price and Carl G. Rosberg. (Berkeley: Institute of International Studies, University of California, 1980. Pp. xii + 376. $8.95, paper.)

Publication of this collection of papers, originally prepared for a conference held in Berkeley in early 1978, provides students of political and social change in South Africa with a solid academic description of recent trends and a few theoretically based predictions about the future. The book, amazingly inexpensive by current standards, is well written for classroom use, and most of the authors have updated their chapters to cover events of 1979. Thus the book makes a substantial contribution by summarizing and updating knowledge and making it available to students. It is unfortunate, however, that greater editorial control was not exercised to eliminate excessive duplication in the material presented in various chapters.

Only a few chapters go beyond summary description to present new data or original analysis. In a chapter entitled "Change in South Africa: Opportunities and Constraints," Lawrence Schlemmer discusses the implications of recent survey research conducted among both whites (a broad sample) and blacks (more specifically, Soweto Africans with at least Standard 8 education) for the possibility of change. He finds that the attitudes of whites, and especially of Afrikaners, have changed on some issues while remaining firm on others. "Soft" issues on which change has taken place include occupational advancement, improvement in the socioeconomic conditions of blacks, integration in sport and other "impersonal spheres of social interactions," and the creation of "overarching linkages between separate political institutions." "Hard" issues on which attitudes have remained constant include preservation of white economic benefits, separation in intimate spheres of interaction, and political integration within common organizations resulting in the elimination of ethnic corporate autonomy.

Schlemmer concludes that this pattern of attitudes leaves "great scope for the type of innovation which will counter the well-nigh total political alienation of urban Blacks. . . ," and that "the time is clearly auspicious for bolder moves than hitherto in the direction of a constitutional arrangement which will embrace all South Africans and will allow for steady evolutionary changes toward a stable, just, and open society with constitutional protection of group interests" (p. 274). He cites in support of these conclusions the finding that 57 percent of the Soweto African sample, although conscious of great deprivation, highly politicized, and almost unanimously supportive of radical political organizations, were "willing to accept and happy to live with" a government having equal numbers of blacks and whites in the cabinet, while only 35 percent said the same thing about a government in which a black majority would rule the whites (p. 277). Furthermore, most African respondents were worried that black rule would create inter-ethnic or economic problems or white alienation and emigration.

While Schlemmer readily acknowledges that inclusion of non-racial rule as an alternative to a balanced cabinet or black dominance would have produced a substantially different response pattern, he does not discuss the relationship of attitudes to possible changes in the power situation in South Africa. While a racially balanced cabinet may be an acceptable alternative to many Soweto Africans today, it is not acceptable to the parties they would elect to the African half of such a cabinet if they were given the vote. And if these parties boycotted the elections leading to a racially balanced cabinet, or were prohibited from participating in them, this constitutional arrangement would surely be deprived of much of its support.

Robert M. Price's concluding chapter, "Apartheid and White Supremacy: The Meaning of Government-Led Reform in the South African Context," presents a more pessimistic view. Distinguishing between those aspects of apartheid which have consummatory significance as ultimate values and those which have instrumental or utilitarian significance as tools for the maintenance of white supremacy, Price suggests that events of the last 30 years, both within South Africa and abroad, have made abandonment of the former aspects both possible and neceessary for the preservation of the latter aspects. Consummatory apartheid has not succeeded in preserving internal security or in gaining international legitimacy. The Botha government has responded to these failures with a pattern of reforms designed to "eliminate those aspects of apartheid which have drawn the most international attention and criticism, while at the same time tightening state control in those areas where apartheid has proved less than fully effective" (p. 316).

Price demonstrates that recent changes in personal apartheid, African trade union organization, influx control and pass laws, rights and living conditions of those Africans who are entitled to remain in urban areas permanently, inclusion of such Africans the emerging pluralistic constitutional formula, and control over mass media all fit into the pattern specified above. He concludes that this pattern of reform will probably not succeed in attaining internal stability and international legitimacy because it does not go far enough to prevent internationally visible, militant black opposition. Such opposition can be eliminated, he believes, only by policies which would substantially diminish white privileges. While the future is always uncertain, this conclusion is probably more realistic than Schlemmer's hope that a stable, just society can be achieved through an acceleration of present trends in political change.

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Resource Mobilization in Poor Countries: Implementing Tax Policies. By Alex Radian. (New Brunswick: Transaction Books, 1980. Pp. xxiv + 266. $19.95.)

The first impression of this book is that Dr. Radian is the Dr. Reuben of LDC public finance, telling us all we might ever want to know about the problems of tax assessment and enforcement in developing countries, but further reading and reflection suggests more eminent literary comparisons. As one considers the journey Radian takes us on through Jamaica, Trinidad, the Philippines and Thailand, with references to experience elsewhere in Latin America, Asia and occasionally Africa, the analogy with Jonathan Swift seems more apt. We are shown how people in these other countries, like the Lilliputians, are outwardly doing things quite similar to what we do, but with quite different results, creating a topsy-turvy world out of familiar objects. They can't seem to do anything right. Efforts to eliminate a backlog of cases create more of the same (pp. 102, 116); collection campaigns don't work (pp. 156-57); the courts let off tax evaders with light penalties even when cases are "air-tight" (p. 189), and so on.

Radian's own description of his first encounter with the tax departments of Jamaica and Trinidad in the summer of 1974 suggests that he started as a latter-day Gulliver. (These departments looked "remarkably like the U.S. Internal Revenue Service. Everything the book says should be there was in its proper place. . . ; [however,] things began to lose much of their glitter when officials were asked to describe how the new system actually works.") Still, more careful reading reveals that his observations are more like those of Yossarian in *Catch-22*.

Time and again the personnel in tax departments are caught in situations where to follow the "book" produces the wrong results. In his explanations, Radian is more like Joseph Heller, chronicling the irrationalities of bureaucratic performance. Responding to the government's call for producing more revenue leads diligent collectors into conflict with government when they press its supporters to pay up arrears. Tightening the assessment noose seems to produce still new opportunities for evasion. Actually, most of the behavior described fits the standards of "rationality," given people's range of alternatives and career incentives. Unfortunately, what pays off for the individual civil servant does not for the government as a whole.

The book usefully elucidates many of the decision rules which account for this behavior, and also suggests decision rules that can produce some positive results (e.g., pp. 124-34). In its methodology and exposition, it most resembles the book by Caiden and Wildavsky, *Planning and Budgeting in Poor Countries* (Transaction, 1980). If you liked that book, you will probably find this one to your liking, if less lively. The rule of "redundancy" figures prominently in the analysis, as might be expected, since Radian and Wildavsky have previously been collaborators (*Budgeting: A Comparative Theory of the Budgetary Process* [Little, Brown, 1975]).

Political scientists as a group are likely to be less pleased with the book than public administrationists because it gives politics shorter shrift than warranted. One of the main themes is that poor tax collection is more a matter of lack of skill than lack of will (e.g., p. 97). While the first lack is easily documented, it is not necessarily a "first cause." While Radian identifies places where political influence and power impinge on the tax-collection process, he shows little curiosity about in whose interest it is to have tax offices understaffed, undertrained, underpaid, not backed up in the courts, etc. While tax auditors told to go after the bigger suspects in the Philippines may indeed find the returns of smaller potential evaders "simpler" to deal with (p. 118), this is not the only reason they shy away from the bigger ones. Only passing attention is given to the political environment which shapes the administration of resource mobilization.

To his credit, Radian does not dwell on "political will," the favored *deus ex machina* of many political scientists, as a solution. Rather, he advocates various practical, incremental approaches to building up compliance (pp. 245-57).

The shape of the larger political system and its bearing on resource mobilization is hardly examined. The most common pattern of government interaction with its various publics in LDCs can be characterized as the politics of largesse. Political leaders, military as well as civilian, autocratic as well as democratic, have been disposed to present themselves to their publics as "giving" without "taking" (though they do "take" for themselves). As long as this is the strategy for justifying continued incumbency, introducing more stringent enforcement of tax laws makes no sense.

Governments do need, of course, ever-increasing resources to be able to play the role of Lady Bountiful, and here they are cross-pressured to lend an ear to suggestions for improving tax collection, if it can be done without much political cost. Radian's suggestions are pitched to such an ear. The logic of LDC political environments helps explain why such a large proportion of revenue comes from *indirect* rather than direct taxation, and it is a shortcoming in Radian's analysis that he devotes most of his attention to defects in the latter.

Perhaps more important is the lack of attention to the expenditure side of public finance in LDCs, the extent to which budgets are prepared with hardly any broad public input. Very special interest groups have substantial influence, and a narrow political elite makes all basic allocative decisions, even when there is a functioning legislative structure. The tax collection procedures and results of the U.S. and U.K. are held up as models of effectiveness (probably too much so, in light of increasing evidence of systematic evasion). But what is not considered is the extent to which American and British improvements in tax collection were made concurrently with democratization of their polities in the latter nineteenth and early twentieth centuries. As the public generally gained more control over patterns of expenditure, so did its willingness to pay taxes. Enforcement mechanisms were strengthened with public acquiescence, as the objects of expenditure more and more met their needs and expectations. Thus, it is more than a matter of political "culture," which Radian does admit into his model of comparative explanation (p. 194).

In the final analysis, resource mobilization in poor countries cannot be treated as a means for their development divorced from the ends to which these resources, once mobilized, would be put. Who benefits (most), and who decides on this, are questions to be explored more extensively. The supply-side and demand-side of public finance cannot be so readily separated. In his treatment, Radian has illuminated many of the factors operating within the structures of administration, and has come up with some well-thought out suggestions for working essentially within those structures. It is left to someone else to show how resource mobilization is a matter requiring much more than implementing tax policies. It comes squarely into the realm of politics—political interests, political leadership, and political mobilization.

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Leaders of Revolution. By Mostafa Rejai, with Kay Phillips. (Beverly Hills, Calif.: Sage Publications, 1979. Pp. 245. $15.00; cloth; $7.95, paper.)

It is unfortunate that theory construction and testing have assumed such priority in political science that some scholars who make other and useful contributions to political knowledge feel compelled to present them under the guise of theory. This work, ostensibly a quantitatively derived or tested (the intention is not clear) theory of the emergence of revolutionary leadership, is really a comprehensive mapping operation of the attributes of a large sample of revolutionary leaders, perhaps the most extensive one since Lasswell's and Lerner's *World Revolutionary Elites.*

In the initial chapters, Rejai summarizes in turn the psychological approach of Lasswell and Wolfenstein, the psychohistorical approach of Erikson and Mazlish, and the sociological approach, equated here with charismatic leadership. Finding each limited in some respects, he then proposes a synthetic theory combining the strong points of all. Revolutionary leadership is asserted to emerge from the interplay or coalescence of three elements: situation, leader motivation and leader skills. The situational element, characterized as crisis, is given primacy; the relevant leader skills are stipulated as verbal and organizational.

That would appear to conclude Rejai's obligations to theory. There is no specification of relationships among or between these elements. That of crisis makes no further appearance. Nor do the components of leadership skills. The rest of this study is devoted to analyses of other data on 64 revolutionary leaders coded for 60 variables hypothesized as relevant.

The 12 revolutions from which the sample is drawn range from the English revolution of Cromwell in the 1640s to the French one led by Cohn-Bendit in 1968. The variables cover: social background; family life; experience, including education, occupation, prison and travel; ideology and attitudes; psychology. Included are such suggestive ones as the number of siblings, and personalities admired by the leaders. For completeness of coverage, one might wish only more reference to sources of the hypotheses and some indication of those with some prior confirmation in the literature.

Frequency distributions of the characteristics yield some interesting results. Well over half the sample of leaders were from mainstream ethnic and religious groups in their societies and had tranquil family lives. Moreover, they were prolific writers, mainly of revolutionary works. Rejai and Philips also provide additional confirmation of findings elsewhere that revolutionary leaders have tended to be intellectuals, relatively young, of middle-class origin. Through factor analysis of the data, five configurations or types of leaders are discerned: founders, agitators, generals, scholars and professional revolutionaries. Half the sample were surveyed for the presence/absence of six clusters of psychological variables held to account for motivations of revolutionary leaders. Findings that over two-thirds were characterized by vanity/egotism/narcissism and by asceticism/puritanism while oedipal conflict was infrequent are fleshed out by illustrative material.

I have reservations about the chapters in which are compared the "classic" revolutionary leaders of the seventeenth and eighteenth centuries and the contemporary twentieth-century ones. Findings to the effect that the former were less travelled than the latter and fewer of the former adopted foreign ideologies tell us more about the independent variable of time period than about revolutionary leaders. I find a similar problem with correlations using level of development of the country as an independent variable. Admittedly, some findings are more interesting than those which tell us that more leaders from undeveloped than developed countries come from rural areas or that all Muslim and Buddhist leaders are found in undeveloped ones. Still, these chapters illustrate the limitations of quantification without a solid theoretical base. Rejai's projected comparison between his sample of revolutionary leaders and one of non-revolutionary leaders should produce more significant results.

Yet even though the current exploration does not add up to the why and how of revolutionary leadership, it does tell us a great deal about the who. Moreover, it provides a data base and interesting observations from which other efforts can be launched.

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Challenge to Governance: Studies in Overloaded Polities. Edited by Richard Rose. (Beverly Hills: Sage Publications, 1980. Pp. 238. Sage research series in European politics, Vol. I. $20.00, cloth; $9.95, paper.)

This book belongs among the growing number of studies that worry about the health of democratic governments of the First World. The list of "the ill" waxes and wanes, but usually includes Italy (for recent examples, see G. DiPalma, *Surviving Without Government* [California, 1977] and A. S. Zuckerman, *The Politics of Faction* [Yale, 1979]); and notably, since the late 1960s, Great Britain (for a relatively early example see Trevor Smith, *Anti-Politics: Consensus, Reform, and Protest in Britain* [Knight, 1972] and for a recent one, S. E. Finer, *The Changing British Party System, 1945-1979* [AEI, 1980]). No Western European government seems immune from such treatment—and if the U.S. is included in the First World, the bibliography of its ailments has become a growth industry by itself. In various ways these studies mark the "crisis" engendered by the gulf between an "ideal" (postulated variously: e.g., according to classic theorists such as Locke, Rousseau, J. S. Mill, Marx; or according to broad concepts such as "participatory democracy," "consensus," "non-alienation"; or bread-and-butter standards, such as "price stability," "full employment," "economic growth"; or public expectations as measured by civic culture types of survey research) and "the reality" (always dismal and described in many combinations of terms: political, economic, psychological, sociological, cultural, ecological, etc.). Conclusions drawn from these "crisis" studies have an equally broad range: From the "need for revolution," to the "need to 'lower expectations,' " (which, in particular applications, may be radical in itself), to the "need for reforms" (of constitutions, parliaments, parties, nominations, elections, policies, policy-making mechanisms, economic priorities, administrative procedures, etc.), to calls for "realism" (or conservatism?) in accepting the way of the world.

Challenge to Governance promises more than it delivers, but because of its topic, it is a worthwhile addition to the literature. The book evolved from a series of conferences and seminars held at various European universities in the late 1970s. Its purpose is "to analyze . . . pervasive challenges to the political authority of [Western] European governments today" (p. 2). All Western European polities are "overloaded," according to this book. Their governments are facing increasing citizens' demands but command inadequate resources (mainly economic, but also political) to satisfy the demands. Hence their publics increasingly perceive them as ineffective. As a result there is a "crisis" in government credibility and this state of affairs threatens the legitimacy (political authority) of every Western European polity.

The book consists of an introductory chapter by Rose on "The Nature of the Challenge" (of overloaded polities) and another by him on survey responses towards respondents' governments, plus chapters by Peter Self on administrative overload in British attempts at planning; by Jacques Leruez on France's indicative planning; by Don S. Schwerin on Norway's experience in comprehensive tripartite bargained incomes policy; by Rudolph Klein on the costs and benefits of administrative complexity to the British National Health Service; by A. F. P. Wassenberg and Jan Kooiman on the fact that while survey research indicates that "overload" is a perceived problem there is no correspondingly drastic decline in confidence over democratic regimes in Western Europe; by Paolo A. Farneti on the Italian response (or lack thereof) to overload; and by Dennis Kavanagh who argues that old-fashioned, heroic leadership (à la Lloyd George, Churchill, DeGaulle) could not solve current problems, nor will increased centralization of decision making.

Although most of the chapters address specific cases, the overall aim is to address general patterns of governments trying to cope with increased demands in the face of declining national economies, inflation, unemployment, increased governmental complexity, and a fall-off of national consensus. The questions raised are: (1) how have governments responded to overload in ways that try to preserve both their effectiveness and popular consent? (2) how do citizens react when they feel there is a decline in their government's effectiveness? and (3) what has happened to governmental authority or legitimacy?

None of the authors reports examples of, or proposes methods for increased governmental effectiveness. The case studies of formal planning in Britain, France, and Norway report inabilities to manage adverse economic conditions. (Yet governments persist in planning efforts because they are *expected* to have some general strategy.) Nor does any of the authors propose structural solutions or normative policies to improve effectiveness; and none advocates an Eastern European "socialist" method of maintaining effectiveness and compliance by adopting coercion.

What then of the future for consent? The authors do not forecast any real revolutionary threat to First-World polities. Where terrorists are active (Italy, France) the public has responded with heightened support for the existing polity (though not necessarily its political leaders). Among proposals to maintain or foster consent, the articles by Klein, Wassenberg and Kooiman, Kaase, and Kavanagh endorse structural reforms towards more widespread participation in decision making to cope with a fall-off of party support and the diffusion of power in modern societies. But admittedly, participatory schemes trade off efficiency in favor of consent, for they increase the cost and complexity of decision making—and may even heighten overload from those who champion contradictory goals.

Further, with regard to consent, Rose reminds us that citizens' choices represent more than a stark polarity between compliance (full consent) and revolution. People can and do exercise the option of "indifference." Tax evasion, famous in Italy, is a mark of civic indifference, a distrust and doubt that government can or will make things better. Refusal by unions and employers to be bound by wage-price guidelines is another manifestation of "indifference"; and Rose points to several other examples (pp. 22-26).

But all this further erodes government authority, for as individuals and groups ignore governmental policies declared to be for the good of the whole, governmental efficiency declines because Western polities have always relied heavily on voluntary cooperation by the public.

My greatest caveats about this work come from the feeling that there are too many generalizations from too few cases, and, more basically, that the notion of "overload" is pretty overloaded itself. Is it *cause* or *effect* of governmental *action* or *inaction*? Perhaps Rose could have added a concluding chapter clarifying the term.

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Peasants of Costa Rica and the Development of Agrarian Capitalism. By Mitchell A. Seligson. (Madison: University of Wisconsin Press, 1980. Pp. xxxii + 220. $21.50.)

Mitchell Seligson's book is about the impact of agrarian capitalism on Costa Rican peasants during the nineteenth and twentieth centuries. Using a variety of techniques including survey research, Seligson traces the evolution of peasant living conditions and attitudes as they were influenced by changes in the nature of the rural economy. He argues that the development of coffee into an export commodity during the nineteenth century gradually transformed the self-sufficient class of yeoman farmers inhabiting the central highlands into a fragmented collection of landed peasants (with and without title), landless laborers, and squatters. Growth of the rural population, lack of adequate land in the central highlands, and the limited number of urban jobs gradually marginalized the yeoman class, forcing many to migrate to less-productive lowland regions.

The major question Seligson attempts to answer is why Costa Rican peasants seldom rebelled, given such downward mobility. He argues that changes in the agrarian sector came slowly, allowing for adaptation. Some peasants were able to find work on the expanding lowland banana plantations. Using survey data, Seligson argues that the primary concern of agrarian workers is not maintaining ties to the land but rather personal security. This being the case, wage employment on the banana plantations was viewed as a relatively attractive alternative. He also asserts that the ethnic homogeneity of the population prevented the kind of cruel and systematic exploitation of the peasant class that occurred in countries such as Guatemala.

While the central hypothesis concerning the relationship between security and rebellion is well argued, several points bear further investigation. First, it is not clear that a self-sufficient yeomanry existed in Costa Rica over a long enough period for a new class mentality to have formed. Seligson notes that the central highlands were settled during colonial times by Spaniards from poor areas but with strong aristocratic aspirations. These poverty-stricken settlers had to wait awhile for the development of an economic base from which to finance these aspirations, but the opportunity finally came with the nineteenth-century coffee boom. Although Seligson entitles his first chapter "The Rise of the Yeoman," it might more appropriately have been called "Limbo for the Aspiring Aristocracy." The Costa Rican claim of cultural and political uniqueness may contribute to a tendency to romanticize rural class attitudes and relationships as they existed during the seventeenth and eighteenth centuries.

Secondly, Seligson does not clearly specify the nature of the elite that emerged during the nineteenth and twentieth centuries. Was this elite a product of the temporarily marginalized groups mentioned above? Were large numbers of nineteenth-century immigrants involved? What role did intermediaries in the coffee export trade play? While the coffee oligarchy is at one point described as having been coterminous with the government elite, this oligarchy's growing impotence with regard to tax policy is also noted. Although lack of more detailed attention to the nature of the Costa Rican elite is understandable in a book on peasants, further knowledge concerning the nature of this elite might have contributed to an understanding of its relationship to rural groups.

Finally, several observations can be made concerning the attitudinal data presented. While the case for peasant concern with security is convincing, Seligson further argues that peasant organizing activities are a response to insecure conditions. If the search for security plays a primary role in such activity, it is difficult to explain the high levels of organization on the "secure" banana plantations, and low levels elsewhere. Seligson also argues from survey data that the agrarian reform programs sponsored by the government agency ITCO have increased peasant feelings of efficacy. However, he further observes that ITCO adopted policies in the 1970s which stressed self-selection of peasants for reform programs. One would expect such self-selecting individuals to express feelings of efficacy when surveyed.

Seligson generally succeeds in presenting a comprehensive view of the evolution of Costa Rican peasant society. His study represents one of the few attempts to combine historical analysis of the impact of agrarian capitalism on peasants with detailed survey research. This book will be read not only by those interested in agrarian problems but by individuals wishing to understand contemporary Central America. It contains insights that should be particularly useful during the tumultuous decade of the 1980s.

STEVE C. ROPP

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Politics and Purges in China: Rectification and the Decline of Party Norms, 1950-1965. By Frederick C. Teiwes. (New York: M. E. Sharpe, 1979. Pp. xiii + 729. $35.00.)

In the absence of a set of formalized rules, procedures, and institutions, management of elite conflict in communist societies has proved to be a difficult problem. In this volume, Frederick Teiwes examines how the Chinese regime has attempted to use the rectification approach that was developed during the Chinese revolution to manage elite conflict during the early post-revolutionary period. In his examination, Teiwes develops detailed balance sheets of the political costs and benefits of the major political campaigns during this period.

Teiwes' major thesis is that a decline in policy consensus among the Chinese elite led to widespread violation of the norms of rectification, thereby setting the stage for the Cultural Revolution. Rectification as a conflict resolution process assumed openness in policy making, obedience in the implementation of policy decisions, and a reasonable degree of leniency in treating violations of elite discipline. As disputes over the direction of policy developed, dissent from the chosen policy direction was treated harshly, with substantial damage to the integrity of the decision-making process and the morale of the cadre as a whole. Teiwes draws particular attention to the contradictory role of Mao Tse-tung as both a strong supporter of the norms of rectification and as a sometime violator of those norms, particularly during the late 1950s and early 1960s.

The book begins with an overview of the doctrine, methods, and historical development of the rectification process. It then proceeds to analyze the major campaigns of the period, starting with the consolidation campaigns of the early 1950s (e.g., the "Three Anti" and "Five Anti" campaigns) and ending with Socialist Education Movement of 1962-1965. A concluding chapter draws together the arguments advanced in the individual chapters and makes some suggestions regarding the implications of the analysis for current Chinese politics.

The strength of Teiwes' study is his painstaking analyses of the major political campaigns of this period. Drawing heavily upon previous research, Chinese publications of the time, and Chinese material revealed during the Cultural Revolution, he meticulously traces the timing, direction, and effects of the campaigns as well as the particular roles of individual Chinese leaders. Despite the often substantial problems of missing data, he manages to develop carefully reasoned judgments on the issues involved. It is worth noting that some of these judgments are contrary to those previously offered by other analyses of these events (e.g., Richard Baum's analysis of the Socialist Education Movement in *Prelude to Revolution* [Columbia, 1975]).

Beyond his balanced analyses of specific political campaigns, Teiwes' study could have benefited from a more systematic attempt to consider China's rectification process in a comparative perspective. The only comparative reference Teiwes makes is to the Soviet Union of Stalin's time. Clearly, this is not the only possible point of comparison in the study of Communist systems. One suspects that using Stalin's regime as the only benchmark biases evaluation of Chinese rectification practices in a favorable direction. The lack of comparative perspective is also reflected in an absence of reference to relevant studies in psychology, sociology, and political science on norms and organizational behavior. Given the stress on rectification as a set of organizational norms for the Chinese Communist party, this is somewhat surprising. The general approach is to treat Chinese politics as *sui generis*. This deficiency is particularly apparent in the discussion of the rectification process in chapter 2. Current literature on the processes of attitude change could have added much to Teiwes' discussion of the likely effectiveness of the rectification process.

The absence of a comparative perspective poses particular problems in trying to assess the efficacy of the rectification approach as a long-term solution to problems of elite conflict in the People's Republic of China. While cognizant of the ambiguity and fragility of rectification norms, Teiwes suggests that in the future these norms can again provide a framework for managing elite conflict, albeit with some irreducible element of unpredictability (p. 633). This suggestion is particularly surprising, given the ample evidence offered in *Politics and Purges in China* that Chinese cadre at all levels have been frequently at a loss in trying to divine what policy means and have often been caught pursuing previous policies after new ones have been established, with disastrous personal consequences. As long as there exist few meaningful "exit" options for dissident or mistaken cadres and there is no institutionalized procedure for insuring Chinese leadership succession, rectification does not seem to provide a particularly viable long-term solution.

The absence of a comparative perspective also makes it difficult to assess the significance of Teiwes' major finding that lack of policy consensus led to a decline of rectification norms. That extreme disagreement about goals should lead to violations of the "rules of the game" on how to handle conflict over those goals seems unsurprising. It would be useful to know whether this pattern is indeed common across different political circumstances, it indicates a relatively low level of institutionalization of the Chinese Communist party, or, as the author suggests, it is a reflection of the unique role of Mao Tse-tung.

In sum, *Politics and Purges in China* is a book that should be read by students of Chinese politics for its analysis of the political campaigns of the 1950s and 1960s. For comparative scholars interested in understanding the Chinese experience in a larger context, it has less to offer.

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Ethnic Politics in Malaysia. By R. K. Vasil. (Atlantic Highlands, N.J.: Humanities Press, 1980. Pp. xii + 234. $12.00.)

This work had its origins in the two well-received earlier publications by the same author: *Politics in a Plural Society* (Oxford, 1972) and *The Malaysian General Election of 1969* (Oxford, 1971). Portions of chapter 3 and most of chapter 4 of *Ethnic Politics* replicate or summarize material from Vasil's first book and pp. 145-78, chapter 5, of *Ethnic Politics* replicate or summarize material from his second book.

Ethnic Politics analyzes developments to 1973, though there is a footnote reference (p. 205) to the 1978 elections. There is an incisive overview of the *Barisan Nasional* (National Front) which emerged in 1972 and controls 131 of 154 seats in the House of Representatives. Vasil's evaluation of the Front, though overly pessimistic in its assumption of permanent and heavy-handed Malay domination, is quite useful. However, there is little substantive information provided on the day-to-day workings or policies of the Front.

Vasil argues the 1957 independence constitution was to lead toward political equality for all races, the special position of the Malays—Article 153—was an interim measure, and that the Malays were not to remain an economically depressed plurality in Malaya, now Malaysia. An underlying theme is that the Alliance comprised of UMNO (United Malays National Organization), the MCA (Malaysian Chinese Association) and the MIC (Malaysian Indian Congress) which evolved between 1951 and 1955 was "a freak of history" based on expediency (pp. 62-63). UMNO accommodation gradually became domination because UMNO Malay leaders always were committed to the principle that Malaysia could not be governed by a mixture of races. On all crucial issues, the Malay position must dominate. A paradox alluded to but never explained, if indeed it can be, is this: from 1957 to the communal riots of May 1969, non-Malay communal parties kept pushing for more political parity. But, if the more moderate UMNO leaders were to compromise, where was the *quid pro quo,* i.e., significant improvement in the Malay economic position? A diminution of Malay political domination had to mean yielding up economic benefits by the Chinese, but this did not occur.

In selected areas, I disagree with Vasil. He is too critical of moderate UMNO leaders. When Tun Razak became prime minister and Mahathir deputy prime minister, their moderation became apparent. Not all MCA leaders were prosperous businessmen. T. H. Tan, for example, became a *towkay* after he achieved leadership status. The "starting-point of the communal holocaust of 13 May 1969" was not so much the victory celebration of the Selangor state UMNO in Kuala Lumpur (p. 182), but the provocative parades by non-Malay partisans who thought they had achieved victory, as well as the 1964 communal riots in Singapore and West Malaysia. The suspension of parliamentary government and the establishment of a National Operations Council on May 14, 1969 resulted not only from pressure by UMNO extremists (p. 184), but also on the advice of most cabinet ministers—Malay and Chinese. Finally, one should not assume the era of accommodation is past and non-Malay parties in the National Front are subservient appendages. The non-Malay parties, no longer limited to the MCA and MIC, represent non-Malay influence and there is room for compromise. For example, Chinese control approximately 29 percent of Malaysia's economic assets today, up from 22-23 percent in 1970. The commitment that Malays will control 30 percent by 1990 will be achieved by an expanding economy and redistribution of foreign-controlled assets rather than at the expense of Malaysian Chinese and Indians.

A fundamental problem insufficiently discussed is the fact that no strong, widely supported Chinese leadership has emerged in the postwar period. Many Chinese businessmen today lack confidence in the MCA; witness the fact all MCA candidates who recently contested the Selangor Chamber of Commerce election were defeated.

Vasil's book provides a good foundation for analyzing contemporary Malaysia. It also will be useful for those interested in Third-World political parties and/or the effort to maintain a modicum of democracy in a communal society.

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The Centralist Tradition of Latin America. By Claudio Véliz. (Princeton, N.J.: Princeton University Press, 1980. Pp. xii + 355. $22.50, cloth; $9.75, paper.)

Claudio Véliz is familiar to Latin Americanists as the editor of two collections of essays on Latin American politics and of the useful *Latin America and the Caribbean: A Handbook* (Praeger, 1968). Now he has undertaken the large task of re-examining the history of Latin America since the Conquest with a view to isolating and analyzing its "centralist tradition." The resulting book is somewhat curious. Its two concluding non-historical chapters, on "Latin American Industrialization" and "Authoritarian Recentralization," are the most successful. In the eleven that precede them he identifies historical *antecedents* for the present-day socio-political phenomena that interest him, but I do not believe that he establishes chains of *causation.*

His working definition is somewhat meagre: "the centralist disposition of Latin Americans ought not to be confused with an ideology. Rather, it can be taken to be a . . . description of the ways in which Latin Americans have gone about attending to their economic, social, and political arrangements. . . . Until now [it] has been latitudinarian, legalistic, and emphatically civilian" (p. 302). So lightly armed, he nevertheless ventures into the historical literature in search of evidence; and not surprisingly he finds it. Though he resorts to the occasional eccentric source or interpretation—the White Legend according to Hanke and others simply will not support the weight he places upon it—most of his readings fall within the conventional wisdom. He places the beginnings of Latin American centralism in the Conquest: in the extraordinary efforts through which the Castilian crown eliminated all rivals, real and imagined, ecclesiastical, civil, and military, to its supremacy in the Indies and imposed there the most advanced colonial bureaucracy of the age. Véliz stresses the continuities that lasted through four and a half centuries—through Hapsburgs and Bourbons, bourgeois republicans and backwoods caudillos, urban populist demagogues and the most recent efflorescence of military saviours. Aware that Brazil's beginnings had little in common with those of Spanish America, he accounts for present-day similarities in political culture by hypothesizing a confluence of traditions in the eighteenth century at the time of the Pombaline reforms on behalf of the Portuguese Crown and the Caroline reforms on behalf of Madrid. The durability of the centralist tradition he attributes chiefly to elements *not* within the Latin American experience: feudalism and its breakup; religious non-conformity; the industrial revolution of the eighteenth and nineteenth centuries; the ideological, social, and political concomitants of the French Revolution.

In general terms, these arguments are familiar enough; in Véliz' hands, however, they lack important nuances. The most serious problems arise from his neglect of the environmental-economic and demographic matrix. He thereby fails to see *dis*continuities created by wildly gyrating boom-and-bust cycles, by protracted contraction of imperial trade and communications in the seventeenth century and the consequent emergence of local and regional networks, by periodic retreats into ruralism and self-sufficiency, by stagnation of social and economic life in the first half of the nineteenth century and its reorientation in the second half by virtue of imperialist penetration, above all by the secular contraction and subsequent explosion of population. Historically there has been ample scope for the exercise of initiative and power at the hacienda, municipio, regional, or interest-group level. Although the ideological and juridical culture has been hostile to legitimization of a sort of *political* pluralism, it does contain strong safeguards for entrenched *social* and *economic* privilege or simply for workable ways of life. Latin America is encrusted with such survivals: on them are reared the structures of corporatism—more properly, *natural* corporatism. It is with these structures that any centralizing regime must interact; the *interaction* gives the distinctive Latin American style. Remarkably, Véliz pays only perfunctory attention to Iberian parallels. A more systematic treatment might have proved illuminating, not least a treatment of the nineteenth-century Spanish experience of economic contraction, centralist/regionalist and modernist/traditionalist struggles, predatory militarism, and penetration by European capital. It is not coincidental that Juan Linz' observations on Spanish authoritarianism offer one of the most useful conceptual guides to Latin American politics.

Véliz is more convincing on modern Latin American industrialization and its consequences. "Industrialization did not come to Latin America as the result of an indigenous process of growth," he writes, "but as a complex response to an external stimulus" (p. 252). He describes in broad strokes the growth of import-substitution from 1929 through the Second World War and discusses the reasons why the state rather than small private entrepreneurs assumed the more dynamic role. He comments perceptively on industrialization's almost total lack of impact on either the popular or artistic imagination of Latin Americans. His observations on the reasons for the survival and prosperity of Latin America's landed oligarchs in changing times are also worth pondering. But Véliz' concluding political analysis is despairing. Magisterially he dismisses nineteenth-century liberalism and twentieth-century Marxism as "exotic" doctrines unsuited to Latin America's unique circumstances. The populism of the 1930-1970 era was unsuccessful for its disastrous insistence on distributing slender and unreliable state revenues rather than aggregating and investing them. He banishes military developmentalism—principally, it would appear, because he detests militarists. Unlike other writers who seek the "unique tradition," Véliz has no sympathy for quasi-fascist or "corporatist" solutions. He is left with bureaucratic centralism, which he describes as "a pragmatic tradition devoid of ideology . . . as much the consequence of accidental external circumstances as of deliberate choice" (p. 301). Like some islet in the Outer Hebrides, it is terrain as narrow as it is bleak.

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Models of Political Economy. Edited by Paul Whiteley. (Beverly Hills, Calif.: Sage Publications, 1980. Pp. 388. $20.00, cloth; $9.95, paper.)

The term "political economy" has lately become fashionable, and a variety of scholars seem determined to describe their work using this label. Since a book entitled *Models of Political Economy* could focus on a variety of topics, it might be well to describe the contents of this volume. The major subsections include such topics as "Modelling Politico-Economic Interrelationships," "The Political Business Cycle," "Politics, Inflation and Economic Policy," "Formal Approaches to the Analysis of Political Systems," and "Strategic Behaviour at the National and International Levels." The foreword to the book, by Richard E. Wagner, portrays the contents more simply by noting that most of the papers examine the relationship between "democracy and prosperity" (p. 5), which would probably have been a more suitable title for the book.

In general, the conception of "political economy" that underlies most of the papers in this book coincides with what has variously been called "public choice theory," "positive political theory," or "formal political analysis." There is nothing wrong with this, but it should be noted for the record that a number of scholars who think of themselves as "political economists" are excluded by this definition. See, for example, various articles in *International Organization* during the last decade.

Two strong points of this collection of essays deserve mention. First, the models of politico-economic relations are carefully drawn and potentially useful to other scholars. Second, empirical data on prosperity and democracy is presented for a number of countries, including Norway, Denmark, Sweden, West Germany, Britain, France, and Japan. Those studying the relationship between political support and economic variables will find much that is useful in this volume.

The weak points of the book concern scope, organization, and timing. The misleading title has already been mentioned. The organizational difficulties probably stem from the book's origin as a series of papers presented at a professional meeting. On the whole, these papers form about as much of a coherent whole as most books with similar origins. The addition of an index would have made it easier to find treatments of the same topic by various authors. The papers were presented at a conference held in the spring of 1978. It is unfortunate that Edward Tufte's *Political Control of the Economy* (Princeton, 1978) was published too late for consideration by most of the authors. There is something disappointing about reading a book published in 1980 on papers written two years earlier when a major relevant book has appeared in the interim.

Overall, *Models of Political Economy* is a useful addition to the rapidly growing literature on public choice theory. The discussion of democracy and prosperity in countries other than the United States should be helpful in broadening the outlook of American students of this subject.

DAVID A. BALDWIN

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International Politics

Reform and Resistance in the International Order. By Ian Clark. (New York: Cambridge University Press, 1980. Pp. v + 202. $29.50, cloth; $8.95, paper.)

The "reform" in the title of this book is of rather modest ambition. It stands for "attempts to have the Great Powers subscribe to limited 'group norms' in terms of which the powers might conduct their management of the international system"; attempts that have been exemplified, for Ian Clark, in the nineteenth century, by the Concert of Europe and, in the twentieth, by the League of Nations. "Resistance," on the other hand, stands for balance-of-power politics. Such a minimal conception of the practical improvement of which the international system is capable is illustrated in the bulk of this short book by four chapters that deal with the international history between 1815 and 1980 as the unfolding and then retreat of these reformatory attempts: (1) 1815-1854, basically the era of the Concert; (2) 1856-1914, a period of power politics; (3) 1919-1939, "Concert without balance" and (4) 1945-1980, return to balance-of-power practices pure and simple, exemplified by deterrence and now tri-polarity.

For Ian Clark, the "reform" in the international order has been neither evolutionary nor cumulative; such progress as might have been achieved in period 1 was given up in period 2; and the brief rally in period 3 did not suffice to make of period 4 anything more promising than the return to balance-of-power conditions characteristic of Europe late in the eighteenth century. He sees the to-and-fro between "reform" and "resistance" producing something like a cyclical pattern of international politics wherein the five-power world of the 1970s, at least as envisaged in the Nixon-Kissinger conception of a few years ago, begins to resemble the five-great-power balance of Vienna (1815). Thus, he claims, we have come full circle.

This account of international history as fluctuating between "concert" and "balance" is seen to illustrate the debate, so familiar to students of international relations, between "idealism" and "realism," one that Clark holds to be roughly equivalent to a difference between optimism and pessimism in international affairs and even to Herbert Butterfield's "whig" and "tory" interpretations of history (1931). Kant's views are offered as an example of the "tradition of optimism" (most recently exemplified, Clark argues, by world-order model projects), and Rousseau's views, as exemplars of the "tradition of despair." The discussion of these debates and traditions is even-handed and brings out the strong points of both positions; it runs smoothly and at a brisk pace, even though the reader is taken through much familiar terrain. However, the historical accounts could do with clearer definitions of both "concert" and "balance" and firmer rules about what it is that makes one period one of "concert" and another of "balance." (As formulated, e.g., by Vattel, 1758, common interests and common principles—hence the group norms of concert—were certainly an inherent part of the balance of power.)

The treatment of evidence tends toward selectivity: missing from the account of period 2 are the important Berlin meetings of 1878 and 1884-85 which would tend to weaken the argument; the review of period 4 is curiously telescoped from the perspective of the late sixties and early seventies and not readily acceptable to one who has lived through all of it. Nor do global wars form any part of this story. The long descriptive chapters have no room for either whigs or tories, whose debates were so crucial to the definition of Britain's world position and its conceptions of the balance of power ever since 1688. And finally, the account suffers because it invites drawing conclusions about world politics from what is, for the most part, a story of Europe alone.

Clark's attempt to combine discussions of international theory with a review of international history is certainly to be welcomed, even though little is said about the interaction of these two "dialectics," it being implied that theory affects history hardly at all. According to the book jacket, "the approach of the book reflects a traditionalist persuasion." That is fine, too, but one does wish that the "traditions" of students of IR theory, of whatever persuasion, went deeper than the Congress of Vienna, and nineteenth-century European diplomatic history. For how is it possible to make judgments about evolution and progress in world politics when one's experience begins in 1815? By the time he reaches the stage of conclusions, Clark admits that his time frame might be unduly constrictive and he wistfully refers to the broad vistas of Adda Bozeman's *Politics and Culture in International History*. Undaunted, he proceeds to declare that international politics has not changed in its operative principles in the past 165 years. As a bald statement, that must surely be incorrect. Global politics in the modern period shows both recurrent patterns (i.e., long cycles of world leadership) as well as sustained evolutionary tendencies, likely based on innovation and learning. But, in any event, Clark does not make it clear why such a large and intricate system as international relations should have undergone such dramatic change in such a relatively short period. Maybe the time has come when an adequate theory of world politics needs to be based upon an analysis of the essentials of the entire human experience with world systems.

The book concludes on a decidedly pessimistic ("doubly dismal") note: "If the present system is no more than a balance one, then it would appear that the search over the past one-and-a-half century for a more highly developed form of international order has, at least temporarily, come to a halt. . . . What is more provocative is that, in the meantime, other variants of international order have been attempted and found wanting. What the present age lacks is any widely-shared vision of the direction future reform should take. The question lingers, should the balance system prove defective for our future needs, where do we go from here?" Hence, whatever the shape of this needed vision of the future, it seems unlikely to emerge from an inquiry organized around the respective merits of optimism or pessimism in international politics.

GEORGE MODELSKI

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Problems of Contemporary Militarism. Edited by Asbjørn Eide and Marek Thee. (New York: St. Martin's Press, 1980. Pp. 415. $32.50.)

No one can deny that the rising menace of militarism should be a serious concern of us all. This concern is reflected in this collection of essays written by a diverse group of scholars. Most of the contributors are united in their conviction that the nature of military influence has now become much more pervasive in the post-colonial period. Consequently, they stress the need for a proper understanding of the international implications of "contemporary militarism."

Two major concepts which appear again and again for explanation, clarification, or definition are "militarism" and "militarization." Marek Thee defines "militarism" as a set of "such symptoms as rush to armaments, the growing role of our military (understood as the military establishment) in national and international affairs, the use of force as an instrument of supremacy and political power, and the increasing influence to civilian affairs"; and "militarization" as "an extension of military influence to civilian spheres, including economic and socio-political life" (p. 15). These definitions are echoed, with variations, in a number of essays. Michael T. Kaler's idea of militarism is "the notion of excess of the growing encroachment of the military over civilian institutions with a concomitant decline in individual freedoms and democratic forms of decision-making" (p. 36), while Jan Øberg's idea of militarism is a "merging of major civil and military interests" (p. 49). However, the principal theme remains the same, namely, that military values are increasingly dominating national culture, education, the media, religion, politics, and the economy at the expense of civilian institutions.

The "contemporary" character of this phenomenon, according to most of the contributors, derives from its global reach and especially its extension to the Third World. A number of contributors view it as essentially a post-World War II development and have tried to present analyses of the various socioeconomic, political, and cultural factors responsible for this development. Although these analyses overlap a good deal, the editors maintain a balance by including a few articles like those of Julian Lider and William Eckhardt who believe that militarism is the inevitable consequence of imperialism and that the abolition of the former depends upon the abolition of the latter.

Much as any peace-loving person is disturbed by the global expansion of weaponry, my problem with most of the essays is that they tend to suggest that the Third World is the only devil and that the developed world shares no blame for the rise of modern militarism. Also, it is not correct to say that contemporary militarism is "contemporary" with respect to the Third World alone. Indeed, it is unfortunate that, despite the inclusion of a couple of articles on the transfer of arms, this phenomenon has not been integrated into the various definitions of contemporary militarism. At least one or two papers should have dealt with how the highly militarized nations use transfer of arms as an instrument of colluding with the political elites of the Third World and thereby contribute not only to the suppression of the rights of the people by those elites, but also to "contemporary" militarism.

Nevertheless, the editors of this volume serve a timely warning to the academic community that unless the alarming danger of new militarism is studied and understood, the situation may well slip out of human control. But the problem of research is only one of the two fundamental problems of contemporary militarism. The other problem, posed in the essays by contributors like Richard A. Falk, Dieter Senghaas and Asbjørn Eide (although their basic orientations differ), is how to control the phenomenon of new militarism in a world in which the dichotomy between national sovereignty and individual freedom still exists, in which the world's common people are still denied the bare necessities of life, and in which, as Ejub Kučuk observes in his article, military organization still seems to be the only way to transcend militaristic values.

MAHENDRA AGRAWAL

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Global Food Interdependence: Challenge to American Foreign Policy. By Raymond F. Hopkins and Donald J. Puchala. (New York: Columbia University Press, 1980. Pp. xvi + 214. $20.00, cloth; $7.50, paper.)

This lucid book's title states its insistent thesis: that nations' fates are linked on the food problem. Troubles will proliferate from the indifference of rich nations, from the autarky of poor nations, from neglect and abuse of multinational institutions. The authors review, one by one, the commercial system and the major national and international programs, concluding that all should be used positively and complementarily. It is the maximizing relationship, modified to achieve political feasibility, which the authors continually seek to specify. The United States, as an initiator of international agendas and as a major exporter of grain and technology, has a large but by no means predominating role. The book apparently is an outgrowth of an analysis prepared for the U.S. Department of State.

Mainly from synthesizing other analyses, Raymond Hopkins and Donald Puchala find that "the global food system has not been performing satisfactorily to solve outstanding problems in recent years" (p. 30). Five problems are posed for world attention: chronic shortages in South Asia, Africa, and elsewhere; disruptive instability in supply and prices; insecurity of crucial food imports; low agricultural productivity in less-developed countries; and chronic malnutrition among the underprivileged in many countries.

The international commercial system, though it embraces only 2 percent of world food and most governments resist its effects, nevertheless imposes the principle of comparative advantage without which, the authors assert, world food production would be too costly and too small. However, governmental intervention is inevitable and often necessary to achieve stability and for other goals. Developing nations' commodity agreements, for example, are defensible in promoting their domestic production though these are by no means beneficial to all. The authors waver in judging the self-sufficiency policy of EEC, whose quotas and dumping affront the international spirit, but whose subsidized production reduces international strains during times of world food shortage.

Hopkins and Puchala grant that developing countries have an increasingly disadvantaged posture in international markets, in part because of discriminatory structures such as tariffs that discourage them from processing the products they export. Concessional sales and grants by exporters, like those in the P.L. 480 program, serve producer interests—developing markets abroad, disposing of surpluses—but such programs are sometimes terminated at the moment world supplies fail, frustrating humanitarian and foreign policy goals. Still, the authors value the "heterogeneous coalition" which makes aid feasible, and they argue the need for much more research to evaluate concessional programs.

The management of U.S. food policy is divided between the Departments of Agriculture and

State, understandably, because both furnish knowledge and represent particular interests; but many improvements are suggested in coordinating the two, in quality of personnel, in congressional oversight, and in less reliance upon private interests for technical knowledge and other resources.

Multinational agencies have many shortcomings—redundancy, inefficacy, inhospitability—yet these agencies do provide representation, information, and legitimacy, and their impulse coincides with that of U.S. policy. Therefore, the U.S. should return to its earlier, thoroughly positive attitude toward multinational effort.

Hopkins and Puchala pose three scenarios: continuation of the current pattern of tandem expansion in production and demand of food; tightening of supplies, which would lead to an unraveling of cooperation and, in turn, production; "surprise bounty" in which the world's considerable potential is brought to fruition. Obviously, the second scenario is a disaster to be avoided at virtually any cost in commitment to world food management; the other two scenarios also pose serious problems of stability and equity which demand strong management. The authors conclude that America's heterogeneous political interests lie wholly on the side of positive food goals: rural modernization, adequate diets, food aid as a right, freedom of information and research, market stability, and, in the last analysis, global food interdependence.

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World Labour Rights and their Protection. By James Avery Joyce. (New York: St. Martin's Press, 1980. Pp. 190. $22.50.)

James Avery Joyce has written a short earnest book about the capacity of international institutions to establish and protect workers' rights throughout the world. The study focuses principally on ILO procedures and efforts, but the activities of other UN bodies and non-governmental organizations are also included. Referring interested readers to his other, more specialized works in this field, Joyce intends this volume for a general audience (there is only incidental use of footnotes, and a set of six appendices containing basic texts and other materials is provided). However, the book suffers from the limited depth of treatment Joyce adopts in examining this contemporary human rights problem.

In the first several chapters, Joyce outlines the international legal and institutional bases for the protection of workers' rights. He describes provisions of the Universal Declaration of Human Rights, the UN Covenants, and other conventions as they pertain to such fundamental entitlements as the free choice of employment, just and favorable conditions of work, and freedom of association. Joyce also details the operation of the ILO system and regional arrangements such as those provided within the European Community. In addition, he traces the development of ILO and United Nations machinery to deal with complaints and violations concerning workers' rights. Joyce's discussions, however, go little beyond manual-like descriptions of principles, functions, and events, leaving aside more analytical considerations such as the political and economic contexts for these legal and institutional developments, and the character of evolving trends or relationships in this area of human rights.

Joyce applauds the work of the ILO, particularly for its activities in support of worldwide labor standards and the principle of freedom of association. He notes that much of the ILO's success stems from its capacity to "combine the precision necessary for formulating an international legal instrument with the flexibility essential for its application in countries of vastly different social structures and degrees of economic development" (p. 60). In discussing the importance the ILO attaches to freedom of association, Joyce points out that the ability to join and form trade unions of one's own choice is often essential for insuring other workers' rights—and for protecting individual civil liberties generally. Indeed, they represent reciprocal conditions, since a "genuinely free and independent trade union movement can develop only under a political regime that guarantees human rights" (p. 67).

Joyce believes that the ILO can continue to make great strides in these standard-setting and supervisory activities, despite the attacks and strains to which the organization has been subject in recent years. He is particularly critical of the now-ended United States' withdrawal from the ILO, rejecting the four-point bases for this decision and arguing that the conditions set for remaining in the organization could never have been realistically met within the specified time limit. In Joyce's view, the deterioration and eventual severing of the U.S.-ILO relationship derived more from domestic American politics than from ILO activities. The difficulties also reflected a broader affliction of U.S. participation in international institutions: the problems and frustrations of adapting to a changing global order.

In a central chapter, Joyce presents detailed records of workers' rights violations in nine countries where the treatment of workers and their organizations has been particularly notorious (Chile, Czechoslovakia, Equatorial Guinea, Indonesia, Palestine, Southern Africa, Soviet Union, Tunisia, and Uruguay). These cases are poignant and compelling portrayals of human suffering. But the manner and organization of their presentation detract from their usefulness in understanding the nature and dynamics of this problem area. By his own admission, Joyce offers no systematic perspective within which to view or explain these cases; nor does he attempt to adduce broader conclusions. Indeed, in several instances, the reader is left wondering about the likely disposition of particular cases.

Joyce asserts that current international institutions—strengthened by the cooperative links that have been forged among human rights organizations—offer increased promise for remedying violations of workers' rights. Greater institutionalization, however, is still needed to insure that national governments respect international standards and rights. Joyce calls for more effective UN complaints procedures, a UN court and high commissioner for human rights, and improved fact-finding, investigatory, and government-reporting machinery. Joyce also notes the importance of national institutions—both governmental and non-governmental—for promoting and protecting workers' rights. He cites the establishment of ombudsmen bodies, now operating in some 38 countries, as particularly effective in this regard.

Joyce is intimately familiar with and committed to the protection of workers' rights throughout the world. However, his book's long and extensive involvement is not put to effective use, since he has chosen to keep his treatment of this crucial dimension of international human rights limited and rather superficial.

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Marxism-Leninism and Theory of International Relations. By V. Kubálková and A. A. Cruickshank. (Boston: Routledge & Kegan Paul, 1980. Pp. xvi + 411. $40.00.)

In their book, Kubálková and Cruickshank profess to conduct two dialogues. Thus far, they maintain, each has been a monologue. The first is a dialogue among those in the Marxist tradition. Kubálková and Cruickshank describe the thinking of Marx and his Marxist-Leninist ("Eastern") successors about how the nature of international relations has evolved over time. The second dialogue is a " 'grand debate' within the theory of international relations, a debate that never was and may never be, because of international relations" (p. ix), to wit, a debate between Eastern and Western theorists of international relations. Kubálková and Cruickshank maintain that Western students of international relations have ignored Soviet (or more generally Eastern) Marxist-Leninist thinking with the result that theorizing about international relations in the West has also been a monologue; they propose to overcome that failing by comparing Western and Eastern approaches to international relations.

Marxism-Leninism and Theory of International Relations has its strong points. Kubálková and Cruickshank are right to take seriously the evolution of Marxist-Leninist thinking about international politics and there are good reasons for students of international relations to juxtapose the dominant paradigms of Western and Eastern international relations. Kubálková and Cruickshank quite properly assert as their central proposition that

> Soviet theory, however oversimplified and overweening, does provide some valuable insights into world politics; but of perhaps greater importance to us is the insight that it affords into Soviet *intentions* in regard to that same world . . . particularly when the nation supplying the information is one of the pacesetters of world politics in our time. . . . These theories are . . . no more "outlandish" or less relevant to a study of world politics than are their counterparts in the West (p. 297, emphasis in original).

Moreover, Kubálková and Cruickshank quite rightly detect some convergence in Western and Eastern approaches to international relations. They capture nicely the increased attention in Soviet thinking to a state-focused (horizontal) analysis of world politics. Likewise, they rightly note the movement in some Western thinking away from a focus on the state as the sole actor in international relations, acting and interacting in a world composed primarily of equals, to an attention to world politics in which relations are to a considerable extent vertical ones between unequals.

These strong points notwithstanding, students of international relations are likely to find the book generally exasperating as I, for one, did. My exasperation was prompted by four considerations. The claims for the book are somewhat pretentious; it really is not the case that Western international relations specialists have ignored Soviet or more generally Marxist-Leninist commentary on international relations. Second, they have the story about the evolution of certain key Soviet doctrinal concepts somewhat garbled. Capitalist encirclement was not declared terminated at the Twentieth CPSU Congress in 1956 (p. 161); it was not Khrushchev who stated that the atomic bomb "does not observe the class principle" (p. 164); it is inappropriate to imply that Stalin was abandoning the notion of capitalist encirclement in the post-World War II years as a result of the formation of the communist bloc (p. 151); Stalin did not, as far as I am aware, ever affirm that wars were " 'fatally inevitable' " (p. 164). Moreover, as a depiction of the nature of Soviet leadership after Lenin it is misleading to claim that after Lenin, "the practice has been established whereby political leaders are no longer at one and the same time by definition top ideologists" (p. 170; also see p. 236). Third, I found the lengthy discussions of epistemology turgid and unrevealing. Finally, I am obliged to comment on the structure of the book. The authors repeatedly digress from the flow of their narrative in ways that are distracting, and it is not to their credit that they forthrightly proclaim their digressions (e.g., "Let us interrupt at this stage. . ." [p. 69]); "The question now raised . . . must wait for an answer until. . ." [p. 76]).

The result of these considerations is that the product is not one which will greatly facilitate a West-East dialogue, real or putative, on the nature of contemporary world politics; or, as Soviet writers would be inclined to express it, about the moving forces in international relations.

William Zimmerman

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Russia and World Order: Strategic Choices and the Laws of Power in History. By George Liska. (Baltimore: Johns Hopkins University Press, 1980. Pp. xii + 194. $14.50.)

George Liska's book mainly deals with the analysis of the present precarious world situation and explores the possibilities for American-Soviet cooperation in maintaining a world order. Liska is concerned with the gradual erosion of American world leadership and the lack of success and determination of the Western Europeans either to create an independent center of world power, or to become full-fledged American partners.

Liska sees the reasons behind the decline in American world leadership not only in the lack of psychological preparedness for this task and in the hazy American objectives for the future, but also in our overconcern with economic issues and a neglect of the politico-military factors. "Unless and until power again includes employable military power, and the qualities that make up civic patriotism in changing circumstances are reunited with power behind far-sighted policy, the delay and decline of the West will become an ever more real possibility" (p. 75).

By identifying the USSR and the Third World as the two main threats to the West, Liska recommends to meet the crisis not only with a closer and more extensive politico-military cooperation with Western Europe but also an amicable liquidation of certain fronts with the West's adversaries. Hence he proposes a qualified partnership with the Soviet Union, arguing that the Third World represents a greater danger to the present world order than Russia. As an incentive for such a cooperation, the U.S. should not oppose the Soviet expansion in those parts of the world where American vital national interests are not affected. This approach, thinks Liska, should prod the Soviet Union to a de-ideologization of its system and a limited concept of national interests, while providing it with ample opportunity to be involved in a "world mission" to which its orientation and some structural designs are geared. In Liska's view, a relaxation resulting from an American-Soviet partnership would enhance the political options among the neglected Eastern European states, and most of them (particularly Czechoslovakia and Poland) would accelerate the processes of Westernization in the USSR (p. 68). The reader is reminded that Russia, in view of its previous historical contacts and ideas of Marxism, is not a newcomer to Western ideas, and that some objective processes inside the USSR, such as social transformation, the nationality question, and "the changing balances between agriculture and industry," would bring it closer to a solidarity with the West.

The problem with Liska's concept of a partnership with Russia is that he does not indicate how it should be implemented inside of the USSR and by whom. To de-ideologize Russia certainly is not a price acceptable to the Russian ruling elite, because ideology still plays an important role in legitimizing the rule of the Soviet establishment. Besides, the present interpretation of Marxism-Leninism in the USSR is essentially anti-Western, and it is inconceivable why the crisis in the Third World should change it. The Soviet leadership views the critical and unstable situation among developing countries as a by-product of the capitalist West which at this stage primarily affects the West. They believe that this situation as a whole works to their advantage. Why, then, should they be motivated to reverse this trend by a partnership with the United States, especially since their selective expansionism anyway progresses according to their own terms.

Liska's theory of historical tendencies and cycles, which has some overtones of Spengler's and Toynbee's orientation, is not very convincing when applied to the role of Russia in history. Liska tries to link Russian naval expansionism in the past and at the present to a natural effort to "move out of her land-locked confines" with progressive implications for Russia's internal evolution (pp. 74-75). It is the irony of modern Russian history, however, that liberalization processes were given their chance when the expansionist policy of the tsars or of the Soviet leaders failed, or when they reached an impasse, and that the successes of Russian expansionism had a tendency to entrench a conservative leadership.

While the modern transformation in the USSR makes this country an important base of science, technology, and skilled personnel, the question of how these assets could be tapped for a peaceful global cooperation remains unresolved in view of an increasingly conservative Soviet government.

In brief, Liska's recent book provides a much better insight into the weaknesses and potentialities of the West than into the Russian readiness or prospects for participation in a world leadership with the West.

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The War Ledger. By A. F. K. Organski and Jacek Kugler. (Chicago: University of Chicago Press, 1980. Pp. xi + 292. $25.00.)

One of the casualties of the empirical revolution in international relations research was the use of power as that field's central organizing concept. Now, after some years of relative obscurity, the idea of power as a central concept in international relations may be on the verge of being "born again." Not the least of the evidence of this rebirth is to be found in *The War Ledger,* where A. F. K. Organski and Jacek Kugler demonstrate that the concept of power can yield useful insights into the functioning of important parts of the international political system. Their purpose is to explore the impact of national power capabilities—as measured in economic productivity—upon the outbreak, outcome and systemic consequences of war, and also to assess the extent to which international politics in the nuclear age differs qualitatively from that of the earlier modern era. The scope of the book is obviously broad, and the authors are able to treat intelligently such a variety of topics within a relatively brief space not only because of their concise prose, but also because their approach starts with hypotheses for which they seek appropriate data rather than a data set from which unconnected empirical generalizations are derived.

In assessing the relationship between power and the outbreak of major-power war, Organski and Kugler find that models of the distribution of power within the system, such as balance-of-power and collective security, are not as useful as the power-transition theory proposed earlier by Organski. Emerging from their findings is the assertion that the two main major powers (or contenders), as measured in economic productivity, are so dominant in international politics that they pull along the rest of the system, including even the non-contending major powers. Moreover, wars between contenders are most likely to occur when one equals or surpasses the other in capabilities. Significantly, they find alliances generally unrelated to the onset of war, but closely associated with its widening and outcome. In considering these findings, however, one must remember that they are limited only to the few intense major-power wars since 1870.

Although their data on the contribution of alliances to the outcome of major wars is somewhat limited, Organski's and Kugler's examination of the outcome of minor-power wars is based upon extensive construction of indicators and analysis of data. Drawing upon several post-World War II minor-power wars, they argue that their outcomes were largely determined by the relative capabilities of the nations involved. As in the case of the major powers, the important capabilities are not the number of tanks or aircraft, but instead the basic economic resources of the nation allowing for the government's ability to extract these from the society. This last factor is critical, since nations with approximately equal economic capabilities may, under regimes of varying effectiveness, produce different resource pools for use in war.

In examining the costs of major wars, Organski and Kugler argue that in the long run (for example, over a 20-year period) the distribution of power (as measured by national GNPs) in the international system is not altered beyond what it would have been had the war not taken place. In supporting this argument, they project the economic performance of various nations which participated in World Wars I and II from the prewar base through postwar recovery. Only in the case of the occupied East European nations did World War II have a lasting negative impact upon growth in GNP.

Finally, Organski and Kugler examine the impact of nuclear weapons upon international politics. By carefully examining the outcomes of international crises and U.S.-USSR arms expenditures, they conclude that deterrence is not operating (at least as it might be expected in the abstract) and that the U.S. and USSR are not locked into an arms race. These last findings are in some respects the least singular of the book; the limits of deterrence have been recognized since the early 1950s and even though all may not agree, over the last several years the absence of a classic arms race has been argued by several observers.

Notwithstanding the care Organski and Kugler have displayed in constructing their arguments, some of their other conclusions may be open to question. Is it really the case that World Wars I and II had little impact upon the distribution of international power? Perhaps, but only if power is defined very narrowly in terms of GNP. It is not without political significance that following World War II Germany was split, Japan was essentially converted to a protectorate, and Britain and France were forced to abandon colonial empires they could no longer afford. Wars may not, as Organski and Kugler argue, drastically erode the productive capacities of nations, but they may well alter the manner in which nations are willing and able to use that power; for example, America in the post-Vietnam era seems far less willing to use military force than it did before the "lesson" of Vietnam. It may be even more important that aggregations of power, as indicated by alliance patterns, shift as a consequence of war.

It also may be asked if Organski and Kugler have given the relationship between war and the balance of power a fair test. Essentially, the balance of power argues that alliances coalesce to thwart aggression. Yet Organski and Kugler, drawing upon evidence from the bipolar era state that alliances are too fixed to function this way, while ignoring evidence that for substantial periods of time prior to 1945 alliances were very fluid.

One may also quibble with some of their other interpretations, but on balance this book is a lucid argument for paying much closer attention to the impact of national capabilities on international politics. The arguments may at times be too deterministic, and hence some conclusions may be carried too far, but the challenge and promise of the basic approach cannot be ignored.

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Conflict Resolution and the Structure of the State System: An Analysis of Arbitrative Settlements. By Gregory A. Raymond. (Montclair, N.J.: Allanheld, Osmun & Co., 1980. Pp. xi + 105. $22.50.)

This book is "a preliminary attempt to apply quantitative methods to the study of international arbitration" (p. 93) in order to "sort out those intuitively plausible but erroneous assertions that abound in the literature on international law" (p. 99). It deserves a careful reading by scholars of international law and of peaceful settlement processes.

Gregory Raymond examines three dimensions of arbitral activity: the proportion of central system states whose disputes are arbitrated in a given period; the proportion of arbitrations that involve major powers; and the proportion of arbitrations that involve disputes likely to arouse large segments of a country's population. He charts these variables over time (Ch. 1); searches for correlations with changing legal norms, concentration of power, status inconsistency, and alliance ties (Chs. 2 and 3); performs stepwise multiple regression on the more promising of those variables (Ch. 4); and investigates the effect of arbitration on the magnitude and severity of war (Ch. 5). Hypotheses are from the literature of international law and politics, often including contradictory propositions.

The analysis in this book sustains Raymond's point that "the current gulf separating legal and scientific scholarship is neither necessary nor constructive" (p. 3). It buttresses the view that a stable concentration of power is more likely to promote the peaceful settlement of disputes than are dispersion or volatility in relative power.

The book also illustrates, however, the difficulties in achieving valid tests of hypotheses about international politics. The "major powers" measure of abritration is not normalized for system size, so it is not surprising that this variable falls as the central system enlarges. The disputes that Raymond considers most important are ones that *arise out of* hostilities; many other issues may be more likely to *lead to* hostilities, and therefore more important from a peaceful settlement standpoint. And several correlations that Raymond considers significant seem rather small or raise questions of causality.

Most importantly, much is left undone. Bipolarity should be tried as a predictive variable. Historical factors should be carefully considered; for Latin American countries may have behaved differently from others, and a few major wars may skew an analysis that uses magnitude and severity of war as dependent variables. Finally, a richer theory is needed to explain a world in which increasing the number of disputes may also increase the number of arbitrations. Raymond's book should be read and absorbed, then improved upon.

EDWARD P. LEVINE

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The Nuclear Non-Proliferation Treaty: Origin and Implementation 1959-1979, Vols. 1 and 2. By Mohamed I. Shaker. (New York: Oceana Publications, 1980. Pp. xxiv + 931. Price not given.)

The world has lived in terror of nuclear Armageddon since Hiroshima. Although there is no way to end this predicament completely, efforts to limit at least the proliferation of nuclear weapons have continued since the United States first broached the Baruch Plan to the United Nations in 1946. The present study is a revision and updating of Mohamed Shaker's doctoral dissertation concerning the origin and implementation of the Treaty on the Non-Proliferation of Nuclear Weapons (NPT). The result is a thorough, straightforward account that must still read like its progenitor, complete with hundreds of footnotes and a text which is even printed so as to look like a typed dissertation.

Shaker, a member of the Egyptian delegation to the Conference of the Eighteen-Nation Committee on Disarmament (ENDC) in Geneva during the important period of negotiating the NPT in 1965-1968, and afterwards a participant in the First NPT Review Conference (1975), launches his analysis with the so-called "guiding concept of the NPT" or "Irish Resolution," which was adopted by the United Nations General Assembly in 1961, and carries it through to the end of 1979. Thus, although this is a generally successful attempt to give encyclopedic coverage to an important subject, additions already are in order.

The Second NPT Review Conference (1980), which met in Geneva from August 11 to September 7 (and thus after the publication of Shaker's work) failed to stem the serious breakdown of international confidence on nuclear matters. The peaceful uses of nuclear energy and the lack of progress towards controlling the nuclear arms race were particularly galling. Although 21 more states have become parties to the NPT since the First Review Conference in 1975, none of the so-called "threshold states" have been numbered in this lot. Those which were were mostly states engaging in few, if any, nuclear activities. Tellingly, the Second Review Conference was unable even to reach an agreement on a final declaration.

Shaker organizes his study around an analysis of the various provisions of the NPT and their implementation on the basis of the five guiding principles contained in General Assembly resolution 2028(XX), November 19, 1965. These five principles upon which the NPT originally was to be negotiated are: (a) nuclear proliferation on the part of either nuclear or non-nuclear powers should be halted; (b) a fair balance of mutual responsibilities and obligations concerning the peaceful uses of nuclear energy, peaceful nuclear explosions, and nuclear security guarantees should exist between both nuclear and non-nuclear powers; (c) the NPT should be a step towards the achievement of general and complete disarmanent, particularly of the nuclear variety; (d) there should be acceptable and workable provisions to ensure the effectiveness of the treaty in regards to international safeguards, universality of adherence, and adaptability to changing circumstances; and (e) the right of states to conclude regional nuclear-weapon-free zones should be ensured.

The result is an exhaustive analysis which does indeed satisfy Shaker's hope that his work will "be of use to governments, international and regional organizations, [and] educational institutions as well as to individual lawyers, political scientists and historians" (p. xxiii). The two volumes reviewed here conclude with a brief overview of the NPT and the ongoing process associated with it. Although the reader is told that "not so negligible steps" have been taken towards implementing the provisions of the treaty, one still cannot help but feel a sense of futility concerning the "failure of the NPT to comply in general with the five principles set forth [above]. . ." (p. 927). In other words, the NPT is not preventing nuclear proliferation.

A third volume will contain appendices, a bibliography, and an index to the first two.

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Soviet Diplomacy and Negotiating Behavior: Emerging New Context for U.S. Diplomacy. Study Prepared for Committee on Foreign Affairs, H. Doc. No. 96-238. By Joseph G. Whelan. (Washington, D.C.: U.S. Government Printing Office, 1979. Pp. lxv + 573. $7.50, paper.)

So controversial have been the style, tactics, and motivations of Soviet negotiators that the U.S. House Committee on Foreign Affairs under Chairman Clement Zablocki commissioned the Congressional Research Service, Library of Congress, to undertake a comprehensive study. Its principal author, Joseph Whelan, is Senior Specialist in International Affairs. The premise of his scholarship is that to know Soviet foreign policy, one must know the unique character and motivation of Soviet diplomatic behavior.

Fearing a reversion to Stalin's postwar era with its breakdown in communications and debasement of diplomacy, Chairman Zablocki affirms in the book's foreword that the driving force behind the study is more than the usual academic inquiry and research: for the U.S. and USSR in the thermo-nuclear age, diplomacy and negotiation are mechanisms for survival.

Vast in its historical sweep—perhaps a bit too ambitious—the study probes back to ancient times up through the nineteenth-century and Russian-American encounters in the pre-Soviet period. Focusing on the period since the Bolshevik Revolution, Whelan stresses five themes: the nature of negotiating behavior, its principal characteristics, elements contributing to its formation, aspects of continuity and change during more than 60 years, and the implications of the record for U.S. foreign policy in the 1980s.

The bulk of the book traces Soviet diplomacy under Chicherin and Litvinov, the enormously complex and detailed wartime conferences with Stalin, the descent into the cold war, the transition to peaceful coexistence with Nikita Khrushchev (including fascinating details on the Cuban missile crisis), peaceful coexistence with Leonid Brezhnev (including extensive chronological analysis of the SALT process), and finally, judgments about how U.S. policy should be informed in future undertakings with the Soviets. The unmistakable emphasis throughout is on American perceptions of Soviet negotiating techniques.

Whelan's conclusions appear to be mixed. On one side, he finds the signs of traditional diplomacy and converging national interests underlying the SALT process more significant than at any time in the history of Russian-American relations. He is even prepared to make an analogy between the SALT process and the Congress of Vienna that kept the peace in Europe for 100 years.

On the other, more problematic side, Whelan detects in Soviet diplomacy with the Third World profound ethical and ideological differences with the United States. The intellectualism of the Soviet diplomatic establishment is also questionable, in his view, because it serves party (CPSU) policy which does not necessarily bode well for the U.S. and the West. Finally, he cautions that both Soviet diplomacy and the skills of its negotiators can be overrated.

Whelan is perhaps best when he punctuates his analyses with candid observations from American notables with first-hand experience. Many of them constitute a veritable "Who's Who" in American-Soviet relations over many years. Included are luminaries from the diplomacy of the forties and fifties, as well as many former ambassadors and contemporary academicians long familiar to specialists in Soviet affairs.

Devotees of easement in American-Soviet relations can be heartened by one of the author's many conclusions: simultaneously with the irrational military arms spiral, the world is entering an era of renewal for diplomacy and negotiations; the Soviets are tending to revert to forms, and even the norms, of traditional diplomatic behavior as understood in the Western tradition.

It is in this context that Whelan's study is so valuable. Notwithstanding cold-war rhetoric on both sides, Soviet military power has always been less than that of the U.S. Today, we simply have little experience with which to anticipate Soviet behavior reliably, now that parity and perhaps even superiority in some military subfields have been achieved. Aside from its intellectual contribution to our understanding of a highly vexatious subject, a detailed record of Soviet negotiating behavior might cause us all to appreciate the problems and prospects of inducing the Soviet Union to accept cognitive coexistence and to enter peaceably and permanently that very international order from which the USSR has felt historically alienated.

Whatever the suspicions, rivalries, and conflicts between the two military superpowers, Whelan signals a new context for U.S. diplomacy. He poses the tantalizing possibility that the overarching imperatives of the interdependent global society of our time now require cooperation in restraining the tensions that threaten to destroy us all. Heady stuff, you will say; but important, richly researched, and very worthwhile, indeed.

All books have weaknesses. This one is repetitious, too long, and somewhat over-organized. Judicious editing in a future edition could transform an extraordinary achievement into a truly great one.

NISH JAMGOTCH, JR.

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Deadly Quarrels: Lewis F. Richardson and the Statistical Study of War. By David Wilkinson. (Berkeley: University of California Press, 1980. Pp. viii + 206. $18.50.)

As the title suggests, this brief book examines, summarizes, and extends the research findings reported in L. F. Richardson's seminal contribution, *Statistics of Deadly Quarrels* (Boxwood, 1960). That posthumously published volume and Quincy Wright's epic *Study of War* (Chicago, 1942) generally are acknowledged to be the great pioneering efforts that launched present-day, quantitative investigations into the causes of war. Richardson's work, though frequently cited, probably has never received the readership it deserves. Its somewhat stilted style of presentation and heavy emphasis on mathematical and statistical formulation undoubtedly have scared away many potential readers. Wilkinson, therefore, has made an important contribution by offering an unadorned synopsis of Richardson's data, methods, and findings.

Wilkinson begins with a few words about the motivation underlying Richardson's work on the causes of war, and then turns to the work itself. He explains the matrices, symbols, and conventions that Richardson used to describe the 315 deadly quarrels that he identified. These quarrels, spanning the period from 1807 to 1952, range from small conflicts involving as few as 250 deaths to world wars in which millions of people died. Wilkinson discusses Richardson's findings as they relate to this continuum of deadly quarrels (or wars). The chapters deal with the relationships among the duration, magnitude, and complexity of wars; the effects of common government, prior alliance, and prior enmity on war participation; and the importance of geographic contiguity, particular nations, economic variables, linguistic and religious factors, militant ideology, arms races, types of political regimes, and power disparity, in causing wars. In addition to presenting Richardson's findings, Wilkinson introduces related findings from the works of other quantitative peace researchers and, in places, himself extends or reanalyzes Richardson's work. In the next-to-last chapter, Wilkinson appraises Richardson's coding criteria, procedures for generating data, and analytic techniques. Here and elsewhere in the book, he favorably contrasts Richardson's efforts with Wright's *Study of War* and J. David Singer's and Melvin Small's *The Wages of War* (Wiley, 1972). The book concludes with an excellent summary chapter, followed by several appendices in which Wilkinson critiques the war lists of Wright and of Singer and Small, explains how he has restructured and recoded Richardson's data to produce machine-readable (computer) files, and presents listings of these files.

Wilkinson does not attempt to offer a comprehensive theory of war causation, nor even a series of competing models. We are left, as we are in Richardson's work, with an array of suggestive findings—findings that point toward the broad proposition that similarity, equality, and cooperation generally make for amity rather than enmity. This, as well as his own predilections, leads Wilkinson to prescribe detente among the great powers and accelerated global economic development and equidistribution of benefits as the route to world peace. He notes that the road to global economic development may be strewn with deadly quarrels, but sees such development as the ultimate panacea. Whether global resources will permit such a solution, he does not say.

Wilkinson calls for increased efforts to develop a rich data base upon which theories of war causation can be tested. He is particularly critical of what he believes to be the overly narrow conceptual base of the Singer and Small work. The criticism seems unduly harsh, and many of the lacunae to which he points have been the targets since 1972 of expensive and time-consuming data-generation efforts by Singer, Small, and their colleagues. Wilkinson also suggests the need to synthesize the vast literature on war and peace that has burgeoned in recent years. Such a project undoubtedly would be a major undertaking. Both of these priorities are commendable, but they face serious constraints in this period of diminished financial support for peace research.

Deadly Quarrels is a brief, lucid, and useful book. Those who are intimately acquainted with the quantitative literature on the causes of war will find it a handy reference. For the less initiated, the book provides a valuable introduction. Where better to begin than with a clear exposition of the work of Lewis Fry Richardson?

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Reference Source

International Encyclopedia of the Social Sciences: Bilgraphical Supplement, Vol. 18. Edited by David L. Sills. (New York: Free Press, 1979. Pp. xxxvii + 820. $75.00.)

A political scientist can derive at least two sorts of pleasure from this supplement. Reading through these 215 entries constitutes a liberal education in the recent history of the several social sciences seen through the exemplary lives and notable works of great scholars. This is as painless, illuminating, and amusing a method for improving the mind in these fields as can be imagined. In addition, political scientists can ponder the decisions that went into the shaping of this volume, and learn some lessons about the authoritative allocation of public notice in our region of the scholarly world. Political scientists are, after all, students of influence, and it is plausible to think that influence is what gets a scholar into a volume of this kind. What sorts of influence? Influence with one's colleagues, surely, an unusual gift for providing them with maps of social reality, or ideas, or techniques, or facts that they find useful in their own work. Influence with one's posterity, perhaps, as well. All the scholars whose lives and works are recounted in this supplement are dead, or were born before the end of 1908. Thus there is a quite proper bias in favor of scholars whose contributions have been preserved in some fashion, either through the piety of their students, or through an impact upon some ongoing body of currently interesting work. Finally, there is influence upon the editors of this compendium, who, though conscientious about soliciting advice, may have been so diligent in this connection that they were thrown back, in the end, upon the limits of their own knowledge, training and intellectual breadth. There is, in fact, evidence of just this sort of influence process at work. A political scientist might more readily appreciate this than social scientists differently situated.

Of the pleasures of self-education, many examples could be mentioned. Here is a fragment from the life of Imre Lakatos, as recounted by his colleague, John Watkins:

> A convinced Communist, he was also very active in party politics. . . . In 1949 he spent a year at Moscow University. In June of that year the terror campaign under the Stalinist Rakosi . . . began. Soon after his return to Hungary in the Spring of 1950, Lakatos was arrested and imprisoned nearly four years. He later attributed his survival to two things. First, his Communist faith never wavered (though this was also true of many who did perish); second, though willing to confess any moral view which his interrogators might require of him, he absolutely refused to fabricate evidence for them (p. 399).

This biographical passage provides a fascinating prehistory—previously unknown to me—for Lakatos' 1973 view (reprinted in his *The Methodology of Scientific Research Programmes* [Cambridge, England, 1978, p. 5]): "Has . . . Marxism ever predicted a stunning novel fact successfully? Never!"

In his account of the career of the late Uriel Weinreich, Columbia's great linguist, William Labov manages to raise and discuss a large number of problems which serve not only to situate Weinreich's work in the ongoing stream of research in the field, but also to give an outsider a vivid sense of the stream itself, its eddies and rapids.

There is no dearth of conscientious and imaginative authors to perform this service again and again. See, for examples, Dennis V. Lindley on the work of statistician Leonard Jimmie Savage, Richard Boyd on E. E. Schattschneider, John Chipman on the economist Fritz Machlup or Kenneth Thompson on the historian E. H. Carr.

Only Cyril Burt, of all the 215 subjects in this supplement, receives a distinctly unfavorable notice. Even J. B. Rhine and William Sheldon, neither of them strangers to scientific controversy, get gentle treatment. Peter Gay chooses to strike a defensive posture in his account of Richard Hofstadter's contributions, and Solon Kimball briefly takes on some critics of Lloyd Warner. In general, however, scientific disputation is not given much space in these pages. An innocent outsider might suppose that academic lives are mostly conducted in serene contemplation.

The various social sciences, as defined by the editors of this volume, are uneven in their possession of a common program of research. This unevenness means that a reader is bound to be less confident of the adequacy of coverage in a protean discipline like history than in more cumulative fields, such as linguistics or statistics or philosophy of science. Apparently also in some fields most old masters appeared in the earlier 17 volumes of the *International Encyclopedia,* and more contemporary figures whose contributions provide much of the current intellectual agenda are too young to evade the cutoff date. Thus the period of coverage for this volume gives the impression of coinciding with a slight trough in psychology, for which the resourcefulness of the editors has not compensated as successfully as has evidently been the case in sociology.

The treatment of political science, I am sorry to say, is in some respects wanting. Scuttlebutt has it that backstairs lobbying was necessary to get such a significant figure as E. E. Schattschneider included at all, in a volume that seemingly has ample accommodation for such welterweights of sociology as Arnold Rose, Emory Bogardus, Stuart Dodd, and others too numerous to list. Harold Lasswell, by any standard a social scientist of major importance, receives a five-page spread, one page less than the attention accorded George Kennan, two pages less than his long-time partner, Myres Macdougall, whose influence is limited to the law, less even than the attention accorded to the criminologist, Sir Leon Radzinowicz. Heroes of sociology receive better treatment: Talcott Parsons, to whose memory the volume is quite appropriately dedicated, receives an effusive 21½ pages; Paul Lazarsfeld 13.

The other political scientists present are: Hannah Arendt, Edward Corwin, David Fellman, Carl J. Friedrich, Pendleton Herring, Bertrand de Jouvenal, Hans Morgenthau, Harold and Margaret Sprout, Leo Strauss (four pages), Carl Swisher, Eric Voegelin, and Quincy Wright. Michael Oakeshott and Denis Brogan are also profiled, but are not charged to our account. This seems to me a minimal representation, omitting, for example, our most important text writer, Frederick Ogg, and Merle Fainsod, who in his way had an intellectual impact at least as significant as Kennan's.

The most awkward omission, and it is comprehensive, is in the field of public administration. No Luther Gulick, no Richard Child, no Louis Brownlow, no Arthur Macmahon, no Wallace Sayre, no Leonard White. Consequently this encyclopedia is blind to the influence of social science on that long chapter of civic and administrative and municipal reform in America that stretches onward from the early years of this century: the growth of civil service, the city manager movement, nonpartisan elections and so forth. A pity. Owners of the earlier 17 volumes presumably can console themselves with the biographies of Charles E. Merriam, Westel W. Willoughby, and Frank Goodnow.

Of course I hope another biographical volume in due course follows this one. Political science comes into a golden age during the next round, and I imagine not even a more narrowminded view of things than is on display here will deny entry to at least some of Charles Merriam's intellectual children and the pioneers of voting behavior studies from places other than Erie County.

Technically this volume is a pleasure to use, and to read. Headnotes giving vital statistics might happily have been employed throughout, but apart from that inconvenience, the editors should be warmly commended for encouraging such a high standard of literacy, and for managing to keep to a minimum the woolly writing and fatuous thinking to which we are sometimes prone when praising the famous departed.

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